Why Methodology?

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Why should economists, or for that matter researchers of any kind, bother with methodology? The simple answer is because it is unavoidable. All research contributions carry methodological presuppositions. Decisions about whether or not to use data, to employ methods of econometrics or any other kind of mathematical modelling approach, to halt an empirical investigation once results of the form expected or hoped for are observed, to contrast results achieved with those of others, to emphasise explanatory power, predictive power, understanding or something else, to aim for generality, complexity, simplicity or parsimony, are all methodological.

The only real choice is whether to be explicit or implicit about methodological presuppositions. And the problem with leaving them implicit and unexamined is that the total outcome is usually much the worse for it, with inconsistencies and other limitations unexposed.

The fact is, though, that amongst economists there is quite a widespread hostility to methodology, certainly as an explicit, systematic and sustained endeavour. Why should this be?

Of course, this is not a universal phenomenon. Moreover, this essay is intended for a volume on economics as practiced in Cambridge (UK) where, over much of the last century or so, methodology has figured quite prominently. Certainly, Marshall never shied away from explicit methodological assessment and commentary. His views on the use of mathematics seem especially well known. And for Keynes, of course, explicit methodological or philosophical analysis (stemming back to his fellowship dissertation *A Treatise on Probability* and even earlier writing) conditioned much of his economics contribution (see Lawson 2003a, 2003b). As I shall indicate below Keynes was especially concerned to keep economics realistic. This immediately distinguishes his explicit methodological priorities from the implicit criteria of most modern day economists who work continuously with conceptions recognised as highly fictitious.

Given the focus of the current volume, indeed, it is perhaps interesting to recall that during the period of the late 1930's (at a time when Keynes was worrying especially about the relevance of the methods being systematised as econometrics -- see below), this methodological ideal was expressly translated into a priority for the proposed institution which would become the Cambridge Department of Applied Economics. Thus in 1939, perhaps sensing that economics at large was becoming increasingly divorced from reality, Keynes wrote to Colin Clark (attempting to persuade Clark to become the new department's director) that "It is very necessary to the lay the foundations for a proper department of statistical realistic economics" (Keynes, 1983, p. 801). A year later Keynes had even adopted the habit of referring to the department just as the "new Department of Realistic Economics at Cambridge" (Keynes, 1983, p. 813).

Of course this Cambridge tradition does not end with Keynes. Methodological reasoning (and specifically a concern with being realistic) also figures prominently in the writings of Kaldor (see especially the Okun lectures, Kaldor, 1985) and of Joan Robinson (see especially her *Economic Philosophy,* 1962; "History vs, Equilibrium" {1974, [1980]}; and *"Spring Cleaning*" {1985 [1980]}), as well as of others.

Further, I think it is not too unreasonable to suggest that one of the more active research seminar series in economics at Cambridge over the last sixtenn years or so has been the explicitly methodologically oriented *Workshop on Realism and Economics (*or *Cambridge Realist Workshop)*. Indeed, this current essay is a response to a request to describe the sorts of methodological contributions that have figured prominently in this latter forum, some of which have been systematised under the head of critical (or even Cambridge) realism (see e.g., contributions to Fleetwood, 1999), a matter to which I turn in due course below.

For good or ill, though, this Cambridge tradition appears not to have too many parallels elsewhere; as already noted, there is little doubt that methodology of an explicit, systematic and sustained kind has long been, and remains, largely neglected if not actively opposed in most of the economics academy. In addition there is no denying that methodology of systematic sort is treated in a less than enthusiastic fashion even by the majority of modern day Cambridge economists. So the question posed above remains pressing almost everywhere in the economics academy: Why the hostility to methodology as an explicit, systematic and sustained endeavour?

It seems to me that there are numerous reasons for it. Many are rather poor, or ill informed ('methodologists [especially critical ones] are ignorant of recent developments in economics', or 'methodologists are weak at mathematics', etc.), and I leave them aside. But some of the reasons regularly given, I believe, are not wholly without substance. These are encouraged by prominent, indeed perhaps the dominant, practices or orientations of certain economic methodologists themselves.

I have two sorts of contribution in mind. There are those methodologists who mostly seek to demonstrate only that the practices of (mainstream) economists are basically fine, to `rationalise' what takes place. The second group seeks to impose criteria and methods upon practicing economists, and typically to insist on criteria and methods determined outside of economics, usually by reflection on the practices of physics or biology, or anyway according to some external sort of philosophising.

Both sorts of endeavour tend to be dismissed by the majority of economists as of little interest. The former sort is ignored because, by design, it seeks to change little if anything, merely to ‘justify’ the status quo. As Blaug notes:

> “Too many writers on economic methodology have seen their role as simply rationalizing the traditional modes of argument of economists, and perhaps this is why the average modern economist has little use for methodological inquiries. To be perfectly frank, economic methodology has little place in the training of modern economists” (Blaug, 1980, p. xiii).

The second sort of endeavour is derided because its practitioners tend merely to assert what ought to be done, with injunctions formulated without any obvious consideration of the needs, or context, of socio-economic analysis specifically. Thus reaction towards methodological practice of this latter type very often takes the form of a demand that the methodological critic first demonstrates that some insisted-upon method works. As an illustration, we can note that a recent discussion of the role of methodology found in the *Newsletter* of the *Royal Economic Society* (see for example Backhouse, 1992; Hahn, 1992a, 1992b) was brought to an abrupt end with the reproduction of the following extract from Irving Fisher's December 1932 Presidential Address to the *American Statistical Association*:

> “It has long seemed to me that students of the social sciences, especially sociology and economics, have spent too much time in discussing what they call methodology. I have usually felt that the man who essays to tell the rest of us how to solve knotty problems would be more convincing if first he proved his alleged method by solving a few himself. Apparently those would-be authorities who are forever telling others how to get results do not get any important results themselves” (Fisher, 1933)

If methodology is to be neither uncritical by design nor externally formulated and overly imposing can it have a legitimate role? This appears to be the central challenge facing those who yet perceive a need for methodology of an explicit and sustained sort. It is a challenge that I believe can be met, and here I want to illustrate (at least one way) how. The approach that I shall defend has been adopted by a number of the contributors to the *Cambridge Realist Workshop,* and underpins the project sometimes systematised as critical realism. It is an approach that holds that methodology or philosophy is best conceived as an *under-labourer* for social theory or science including economics. I start by indicating what I mean by methodology as under-labourer. In due course I shall attempt to demonstrate that philosophy so understood meets an urgent need in economics at this juncture.

The under-labourer conception

The interpretation of philosophy or methodology as an under-labourer for science can fairly be attributed to Locke. It is found, albeit almost as an aside, in the ‘Epistle to the Reader' of his *An Essay Concerning Human Understanding*, where Locke writes:

> "The commonwealth of learning is not at this time without master-builders, whose mighty designs, in advancing the sciences, will leave lasting monuments to the admiration of posterity; but everyone must not hope to be a *Boyle* or a *Sydenham*; and in an age that produces such masters as the great *Huygenius* and the incomparable Mr. *Newton*, with some others of that strain, it is ambition enough to be employed as the under-labourer in clearing the ground a little, and removing some of the rubbish that lies in the way to knowledge" (Locke, 1690 [1947], pp. xlii, xliii).

As this extract makes clear, under-labouring for science is not the same as doing science. Nor is it the task of the philosopher as under-labourer to identify sets of rules for scientists and others to follow. I have already noted that not all methodologists accept this. Some do sometimes seek the role of `master-builder' instructing on how economics must be done. Even many econometric texts and courses are like this, insisting on definite procedures or strategies for practice. But methodological injunctions of this kind are no part of the under-labourer conception of philosophy.

My own suspicion is that there are few, if any, valid context-independent rules for science, and that those who attempt to lay down such rules for economics are being rather *un*helpful. But if there were valid universal rules to govern scientific practice, the activity of elaborating them would be more akin to instructing in the more basic techniques and skills of house building, rather than ground clearing. Ground clearing is an activity that happens before most of the paraphernalia of house building even begins to be brought in.

In fact, once the ground has been cleared, the builder may find that there exist possibilities or constraints that direct the building project in previously unimagined ways. And so I believe it is in science.

Now some may suspect that if such under-labouring on behalf of science, including economics, was once necessary, this is no longer so. That is, some observers may suppose that in this post Enlightenment epoch, the entire scientific ground has long since been cleared of its rubbish. Perhaps Locke even faced such a reaction in his own time, at least in connection with natural science. After all, he was suggesting that philosophy had a useful role still to play in the face of the then recent scientific achievements of Boyle, Sydenham, Huygenius, Newton and others. Such was the astonishing nature of some of these achievements that many may have felt that science was in need of help from no activity other than itself. Certainly Locke appears to have anticipated such a response, for he is quite defensive about advancing his under-labouring endeavour:

> "It will probably be censured as a great piece of vanity or insolence in me, to pretend to instruct this our knowing age: it amounting to little less, when I own that I publish this *Essay* with hopes it may be useful to others" (p. xlii).

Censure of the sort that Locke anticipated is well known in economics, of course, as the passage by Fisher noted earlier clearly illustrates. But we can also see that Fisher's observations miss the point of the methodology as under-labourer conception. At least they do so if the ‘knotty problems' Fisher has in mind concern specific substantive issues, or if he imagines that methodology is restricted to giving dictates rather than offering supportive insight. In any case, it is clear that the spirit of the piece, whatever its target, is of the sort that Locke was anticipating.

Aware of the possibility of negative reactions, but not wanting to claim false modesty by pretending his contribution was less useful than he hoped and anticipated it to be, Locke interpreted the nature of his contribution as modestly or unassumingly as he could without undermining his assessment of its worth:

> “I shall always have the satisfaction to have aimed sincerely at truth and usefulness, though in one of the meanest ways” (p. xlii)

Why is his way of seeking knowledge one of the `meanest'? Three hundred years ago the term signified something that is less than noble, unimposing or undistinguished. Here Locke was clearly comparing his role to that of the (noble) `master-builders' of science whose `mighty designs', he anticipated, would leave `lasting monuments to the admiration of posterity'. Locke was interpreting the contributions of scientists as being in some ways or sense superior to his own, but doing so in a manner that did not undermine the worth of his own contribution.

Locke's strategy is a reasonable one, I believe, for modern-day methodologists concerned with philosophy as under-labouring, particularly in the context of economics. If it will help deflect criticism of those who expect philosophy to deliver on the field of substantive theorising or science such a description will serve a useful purpose. I, for one, am happy for philosophy as under-labouring to be regarded as a mean way of pursuing truth and usefulness. To so describe it, of course, is not to render it necessarily without value or inefficacious. Indeed at this moment in time I believe a strategy of under-labouring, in the context of modern economics, promises to be more worthwhile and efficacious than most, at least if an explanatory successful economics is the ultimate goal.

Why do I suggest this? Locke was aware that, despite the then recent successful contributions of Newton and others, (natural) science could always benefit from philosophy. He was worried, though, that the successes of science would engender a philosophical complacency. My own view is that modern economics has not come close to achieving explanatory successes sufficient to encourage a spirit of philosophical complacency. As Ariel Rubinstein (who is hardly a heterodox critic of modern economics) acknowledges:

> “[E]conomic theory has not delivered the goods. Predictions from economic theory are not nearly as accurate as those by the natural sciences, and the link between economic theory and practical problems... is tenuous at best. Economic theory lacks a consensus as to its purpose and interpretation. Again and again, we find ourselves asking the question `where does it lead?' " (Rubinstein, 1995, p. 12).

I shall argue that the reason for this lack of clear direction and obvious progress in modern economics is not that economists cannot make significant contributions (in my own assessment Smith, Marx, Veblen, Hayek and Keynes are amongst those who have done so previously) but that nowadays, at least within academic faculties of economics, the ‘rubbish that lies in the way to knowledge’ has become piled so high that (successful) economic science is (momentarily) well nigh impossible without a good deal of the litter first being cleared away. If under-labouring or ground clearing was ambition enough for Locke in his day, I believe it is an ambition bordering on necessity for any modern day social theorist concerned that there be a fruitful academic discipline of economics.

Ways of philosophical under-labouring

How might we begin to clear a patch of ground? There will always be many ways of proceeding depending, of course, on the nature of the perceived `rubbish'. In modern times a real housing site may be covered in weeds or brambles. But equally it may have parts of old cars strewn across it. The nature of the problem bears on the sorts of ways it may be solved, on the sorts of ground clearing strategies that could be useful. So the first task is to identify the nature of the `rubbish' that is to be cleared away. It is essential always to recognise that the concrete details of the situation will bear on the procedures most usefully adopted. Even so, at an abstract level, it does appear possible to distinguish broad orientations that philosophising as ground clearing might take. Let me briefly consider three such possibilities.

One approach starts from the recognition that our commonsense or everyday thinking includes inconsistencies as well as unreflected-upon assumptions, superstitions and prejudices, which do not withstand close scrutiny. These, however, may bear significantly in the process of science (like much else). Kant argued that it is a function of philosophy to analyse concepts and ideas that are already given but confused. The aim of philosophy, on this conception, is to free up science and other knowledge activities by exposing, criticising and explaining the unsustainable assumptions, inconsistencies and confusions these may contain.

A second approach seeks to inform the scientist of the nature of scientific (and other) contributions to knowledge, and epistemic states of affairs, both within economics and across the disciplines. It is to help the researchers understand where they stand in the wider field of knowledge producing activities, and to help make them aware of potentialities they might explore. I recognise that this second approach can easily collapse into one in which the philosopher becomes a dictator or `master builder’ rather than an under-labourer. For, the activity of pointing to ways of proceeding that have proven successful in some domains, all too easily slides into the generalisation that such procedures are everywhere appropriate, so that economists too *must* utilise them. But this step or `slide’, though easy to make and common enough, is unnecessary. On the approach I am here discussing the orientation to most if not all procedures prior to specific analyses is modal rather than injunctive, that is to suggest possibilities rather than to legislate how it must be..

A third approach is to seek to employ philosophy in the form of logic and argument to dissect and better understand the methods which economists or, more generally, scientists adopt, or, at least as significantly, could use, with the intent of refining the methods on offer and/or to clarifying their conditions of usage.

No doubt there are other ways of philosophical under-labouring. But pointing to these three roles, broadly the *demystifying*, *informing*, and *method-facilitating* functions, should give something of an indication of what I have in mind here.

The context of modern economics

If, as I have observed, specific methods of philosophical under-labouring cannot be determined prior to understanding the nature of the `rubbish' that needs clearing away, a parallel insight holds for science. That is, it is not possible to determine the scientific method that it is appropriate to employ for a given task (in a particular context) without knowing the nature of the task. And to know the nature of any scientific task it is always essential to have an insight into (i.e., to seek to determine) the nature of the material that is to be investigated. Marx once observed that 'in the analysis of economic forms neither microscopes nor chemical reagents are of assistance' (1974, Capital, vol. I, p. 90). His point, of course, was that the nature of the subject matter in question is such that the noted tools are not appropriate to its investigation. But the point being illustrated is a general one. The properties of material studied always make a difference to how we can and cannot know it.

Now if there is one feature that provides *the* greatest obstacle in the path of economics achieving its potential as an explanatory endeavour, in my assessment, it is precisely a failure to recognise the point just emphasised. It is a limitation of modern economics that its practitioners mostly proceed from the idea that the methods of the discipline can be determined independently of considering the nature of its subject matter. I do not mean by this that modern economists experiment with various methods seeking to `select' those that turn out to be most appropriate to the material being investigated. Rather certain methods are insisted upon and treated as more or less universally applicable, without much, if any, consideration of context of analysis. Indeed, even what counts as economics is defined in terms of method. And, of course, the method (or set of methods) that so many regard as essential to, as effectively defining of, economics is that of mathematical-deductivist modelling. Consider the recent observations of Richard Lipsey:

> "[…] 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).

Or, for a heterodox observation consider the experience of Diana Strassmann, the editor of *Feminist Economics*:

> “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.
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> 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.
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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?”” Strassmann, 1994, p. 154).

A previous Nobel Memorial Prize winner in economic science gives a more critical report:

> “Page after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions.....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 without being able to advance, in any perceptible way, a systematic understanding of the structure and the operations of a real economic system” (Wassily Leontief, 1982, p. 104).

I believe it is the orientation of the discipline captured by these (common) experiences that constitutes the primary source of `rubbish that lies in the way to knowledge’ in modern economics. The `rubbish' in question, I hasten to emphasise, is not (of course) the practices of mathematical modelling *per se*. Rather it is the dogma that nothing (or almost nothing) else counts. It is the presumption, which is more or less an edict, that without a model a contributor does not deserve serious attention. It is the rejection of methodological pluralism. It is the idea that, whatever the context, deductivist formalism counts before all else. If this, then, is indeed the dominant form of modern economic `rubbish' the question is how best to clear it away?

Perhaps the first step is to understand this orientation. There are beliefs bearing on the issue that do the rounds in economics and (even if not necessarily wholly compatible with each other) work to sustain the noted orientation. These include assessments that mathematical modelling is essential to science and all serious study, that mathematics is just a neutral language, that there are no alternatives approaches or methods appropriate to conditions addressed in economics, and so forth.

Now it is my contention that methodology can demonstrate that such contentions and others like them are false. If this is so methodology is clearly seen to make a difference. For to the extent that the unreasonable insistence on mathematical modelling is sustained by the belief that the adoption of formalistic practices is a necessary path to science, and/or does not introduce unnecessary constraints on what can be achieved and/or provides a way of proceeding for which there are no viable alternatives, the result can be a rejection of the mathematical straightjacket or, to switch metaphors, an instant clearing of some of the rubbish blocking the possibility of an (or a more) efficacious economics.

Ontology

How are methodological results such as those outlined achieved? Central here is the contribution of ontology. By ontology I mean enquiry into (or a theory of) the nature of being or existence. It is an endeavour concerned with determining the broad nature, including the structure, of reality. Here I am especially concerned with the nature of *social* reality, with the question of social being.

To see how ontology can make a difference in the manner suggested it is important to consider two of the roles that can be accepted for ontological analysis. First we must recognise that specific methods and criteria of analysis are appropriate to the illumination of *some* kinds of objects or materials *but not others*, that the properties of material studied will always make a difference to how we can and cannot know it. I have already emphasised this; it is a failure to recognise this point that is a fundamental problem of the discipline. One role for ontological enquiry, then, is to determine the (usually implicit) conceptions of the nature and structure of reality presupposed by the use of any specific set of research practices and procedures. Equivalently, it can identify conditions under which specific procedures are relevant and likely to bear fruit.

A second, equally fundamental, role for ontology is the elaboration of as complete and encompassing as possible a conception of the broad nature and structure of (a relevant domain of) reality as appears feasible. The aim is to derive a general conception that seems to include all actual developments as special configurations. Put differently, a central objective is to provide a categorical grammar for expressing all the particular types of realisation in specific contexts.

The results achieved by ontology in each of these roles can be used in numerous ways. But of particular interest here is a recognition that the results achieved in these two roles can be used to especially good effect in combination. For if, by employing ontology in its second role, we can achieve a general framework, this can reveal the particularity of many scientific and practical ontologies revealed by employing ontology in its former role. In other words, applying ontology in both of the roles discussed allows us to compare the ontological presuppositions of specific methods with our best account of the nature of social reality. The application of ontological insight in this fashion can reveal in particular both the error, and the non-necessity, of universalising any highly specific approach or stance *a priori*. Ontology, so fashioned, can identify the error of treating special cases as though they are universal or ubiquitous.

Now amongst the results systematised within the realist project referred to above the following are of relevance here:

(i) The sorts of conditions presupposed by the sorts of mathematical-deductivist methods that economists insist upon are actually very rare in the social realm. Certainly they constitute a very special case or configuration of social ontology. As such it is not surprising that *a posteriori* such methods are found mostly to fare rather poorly, that economists who rely upon them are found to have lost their way.

(ii) The sorts of conditions presupposed by the sorts of mathematical-deductivist methods that economists insist upon are actually quite rare in the natural realm too. In contrast the sorts of conditions that are presupposed by and appear essential to the broad variety of *successful* natural science practices are characteristic of the social realm too. Thus although methods of mathematical modelling of the sort economists insist upon seem to be highly limited in their usefulness to social analysis, it remains feasible that a broadly non-mathematical economics can yet be not only explanatorily successful but scientific in the sense of the successful natural sciences.

Notice that these two contentions are formulated in terms of possibilities; there are no stipulations involved. But if they can be established they certainly bear in a relevant way on decisions concerning the sensible orientation of the discipline. Unfortunately, I do not here have space to justify these two contentions fully. But I can briefly sketch something of the relevant arguments, starting with an account of the (rather special) conditions presupposed by the typical methods of modern mathematical economics.

The mathematical-deductivism of modern economics

Note, to begin with, that the sorts of formalistic methods that economists wield mostly require, for their application, the existence (or positing) of event regularities; they presuppose the occurrence of *closed systems*, that is systems that supports regularities of the form `whenever event (or state of affairs) *x* then event (or state of affairs) *y*'. Mainstream economics is a form of *deductivism*, where by deductivism I just mean any form of explanatory endeavour that assumes or posits or constructs regularities (deterministic or stochastic) connecting actualities such as events or states of affairs. It is explanatory endeavour in which closed systems are an essential component.

The fact that formalistic modelling methods require the identification or construction of event regularities is well recognised by mainstream economists (see e.g. Allais, 1992). But the ontological preconditions of these methods do not end there. 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 that 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.

Atomism, then, is essential, if closures of the sort economists usually require are to be assured. However, even in the noted scenarios the assumption of atomism is not yet sufficient to ensure closure and facilitate deductivist explanation/ and prediction. For even with an atomistic ontology, the total effect on an outcome of interest may be changed to almost any extent if all the other accompanying causes are different. That is why, in concrete economic analyses, the (atomistic) individuals tend to be treated as part of an assumed-to-be isolated and self contained set or system.

I am not the first economist located in Cambridge to make such an analysis. Although Keynes never used the term ontology he identified the implicit presuppositions of the by now standard procedures of econometrics almost before they first saw the light of day. But actually it was many years earlier that Keynes first noticed that an atomistic ontology was an implicit presupposition of inductive methods. At the time his main concern was the practices of natural science. Thus he wrote in his *A Treatise on Probability*:

> "The kind of fundamental assumption about the character of material laws, on which scientists appear commonly to act, seems to me to be much less simple than the bare principle of uniformity. They appear to assume something much more like what mathematicians call the principle of the superposition of small effects, or, as I prefer to call it, in this connection, the *atomic* character of natural law. The system of the material universe must consist, if this kind of assumption is warranted, of bodies which we may term (without any implication as to their size being conveyed thereby) *legal atoms*, such that each of them exercises its own separate, independent, and invariable effect, a change of the total state being compounded of a number of separate changes each of which is solely due to a separate portion of the preceding state. We do not have an invariable relation between particular bodies, but nevertheless each has on the others

its own separate and invariable effect, which does not change with changing circumstances, although, of course, the total effect may be changed to almost any extent if all the other accompanying causes are different. Each atom can, according to this theory, be treated as a separate cause and does not enter into different organic combinations in each of which it is regulated by different laws" (1973a, pp. 276, 277).

Note that in drawing attention to this assumption of atomic character of natural law, Keynes is simultaneously raising the logical possibility that not all natural phenomena need be atomic:

"The scientist wishes, in fact, to assume that the occurrence of a phenomenon which has appeared as part of a more complex phenomenon, may be *some* reason for expecting it to be associated on another occasion with part of the same complex. Yet if different wholes were subject to laws *quâ* wholes and not simply on account of and in proportion to the differences of their parts, knowledge of a part could not lead, it would seem, even to presumptive or probable knowledge as to its association with other parts. Given, on the other hand, a number of legally atomic units and the laws connecting them, it would be possible to deduce their effects *pro tanto* without an exhaustive knowledge of all the coexisting circumstances" (1973a, pp. 277, 278).

In the light of this early analysis it is not surprising that many years later, in the late 1930s, when focussing on the social realm specifically, Keynes points out that the same implicit ontology of atomism is a presupposition of any reliance upon econometric methods of the sort being proposed. Thus in an initial response to the League of Nations' invitation to review Tinbergen's work introducing econometrics in a specific context, Keynes writes:

"There is first of all the central question of methodology […]. 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" (Keynes, 1973b, pp. 285-6)

In other words, Keynes argues that so long as we are dealing with an isolated (or equivalently a complete) set of atomic entities we may have some confidence in the method of econometrics. Otherwise there are likely problems. This is more or less the position defended above. Whatever else is the case it is not that mathematical modelling methods of the sort economists use represent just a neutral language. Rather they impose a structure, or more accurately, require of any reality to which these methods are applied, that this structure holds. Basically the standard practices of econometricians, or of economic modellers more generally, are appropriate in conditions were atomistic factors operate in isolation from the effects of countervailing factors. By insisting that that mathematical methods are everywhere utilised in social reality, the presumption is that the just noted conditions are ubiquitous, indeed the sole constituents of social reality.

I emphasis, though, that I have not yet indicated precisely why I am suggesting the modern mainstream tradition fares so poorly as an explanatory endeavour. I have merely indicated that if the methods of mathematical deductivist modelling (as employed in modern economics) are insisted upon as universally valid for the social realm, a presupposition (and requirement for guaranteed success) is that the social realm everywhere comprises (closed) systems of isolated atoms (and noted that Keynes saw this as well).

Now it is immediately clear, I think, that these latter conditions *need* not characterise the social realm. I have elsewhere argued, indeed, that the noted conditions for closure 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. I do not have the space here to derive this social ontology. But let me say something of its method of derivation, and of the sorts of results that are achieved.

Transcendental argument

The point of departure adopted in ontological analysis, at least as I have pursued it, is to suppose that all scientific and other practices, *whether or not successful on their own terms,* are *intelligible*, that they have explanations. This might be called the *principle of intelligibility* (Lawson, 2003a). According to it, there are conditions that render practices actually carried out (and their results) *possible*. Thus, one strand of the strategy followed is just to seek to explain (aspects of) certain human actions, to identify their conditions of possibility. Or, more precisely, it is to explain various *generalised* features of experience including human actions, and so to uncover generalised insights regarding the structure or nature of reality. This of course, is precisely an exercise in ontology.

The principle of intelligibility, that is the initiating presumption that human social activity is intelligible, should not be especially contentious. We all grant it. It is difficult, for example, to imagine anyone bothering to attempt to read and understand these lines that supposes or claims otherwise.

In addition premises of the sorts of (ontological) analyses to which I refer usually express certain fairly generalised features of experience. The form of reasoning that takes us from widespread features of experience (including here conceptions of generalised human practices, or of aspects of them) to their grounds or conditions of possibility, is the *transcendental argument.* The transcendental argument (or transcendental `deduction') is thus clearly a special case of the *retroductive argument*, where the latter moves from conceptions of specific phenomena at any one level to hypotheses about their underlying conditions or causes (see Lawson, 1997a, chapter 2; or Lawson 2003a, chapter 4).

Any results achieved by way of transcendental reasoning are clearly conditional. They are contingent upon the human practices selected as premises and our conceptions of them, as well as upon the adequacy of the transcendental argument employed.

Moreover it is clear that philosophy so conceived, i.e., as method turning centrally upon the transcendental argument, considers the *same world* as the sciences, and indeed serves, in its insights, to complement the latter's results. However, it proceeds on the basis of pure reason (albeit exercising it always on the basis of prior conceptions of historically rooted practices) and produces (fallible) knowledge of the necessary conditions of the production of knowledge.

A theory of social ontology

As I say, in the realist project to which I and numerous others have been contributing, a social ontology is derived by way of transcendentally inferring the social conditions of human practices. I cannot elaborate the numerous arguments here, and refer the reader to Lawson, 1997a, 2003a. However I can briefly summarise some of the results obtained.

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 that 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 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 *internally related*. Aspects or items are said to be internally related when they are what they are, or can do what they do, in virtue of the relation to others in which they stand. Obvious examples are 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. 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, then, is highly internally related or `organic'.

The social realm is also found to be *structured* (it does not reduce to human practices and other actualities but includes underlying structures and processes of the sort just noted and [their] powers and tendencies). And the stuff of the social realm is found, in addition, to include *value* and *meaning* and to be *polyvalent* (for example absences are real), and so forth.

This broad perspective, as I say, is elaborated and defended in Lawson (1997a, 2003a). But I doubt that, once reflected upon, the conception is especially contentious. Nor in its basic emphasis on organicism or internal-relationality is it especially novel, as we have already seen. However, it should be clear that if the perspective defended is at all correct, it is *prima facie* quite conceivable that the atomistic and closure preconceptions of mainstream economics may hold not very often at all.

This, I might recall, was the gist of Keynes' worry, as is clear if I reproduce a fuller version of his assessment extracted above:

> "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" (1973b, 285-6)

Notice, though, that once, of if, the ontology sketched above is accepted, the possibility of social closures such as 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 social events. Keynes is correct to emphasise that there "is no reason at all why [econometric coefficients] should not be different every year". But still it is impossible to assert that they could not be constant (anymore than it is impossible to insist that a fair coin cannot show 100 heads on a 100 throws). However, 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. And to the point, if the social ontology sketched above does not altogether rule out the possibility of social event regularities 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.

Actually the ontological conception sketched here is more explanatory powerful still. For not only does it explain the widespread continued explanatory failures of much of modern economics over the last fifty years or so, but also it can account for both (i) the *prima facie* puzzling phenomenon that mainstream economists everywhere, in a *manner* quite unlike researchers in other disciplines, suppose that (acknowledged) fictionalising is always *necessary* (with human beings theorises as isolated atoms) and (ii) the types of conditions that prevail when mathematical methods in economics achieve such (limited) successes as are experienced. However I do not have space to explore this further here (see especially, Lawson, 2003a).

Natural Science

I mentioned above that ontology can indicate not only that mathematical methods or the sort used by economists are neither neutral nor generally applicable but also that the sorts of conditions which facilitate explanatory successes in natural science are seemingly available to social scientists, including economists, too. Although I must be especially brief here (but see Lawson, 1997, 2003a.) I now seek to provide some grounding for the latter part of this contention.

In essence, my claim is that *if* there is a central component to successful natural science it is *not* reliance upon mathematical techniques but rather the movement from a phenomenon of interest at one level to an account of causal conditions lying at a different level. It is the explaining of falling leaves in terms or gravitational and aerodynamic tendencies or whatever, or the explaining of the symptoms of mad cows disease in terms of the prion, etc.

I hope that this claim will immediately be seen as having some validity. As I say I cannot go into the matter deeply here. Instead, I content myself with considering only (what I believe to be) the best argument against my contention, and showing that it actually presupposes it. I have in mind the response that, whatever the extent of natural science, the one sure component of it is the successful well-controlled laboratory experiment. And experimental activity supports the image of science accepted by the formalistic modellers of modern economics. For in the well-controlled laboratory experiment, event regularities (or closures) of the relevant sort are often sought after and even achieved, and where the latter is so forms of formalistic-deductivist modelling are indeed facilitated.

I acknowledge the import of observation. But, it is important to consider more closely what goes on in such conditions. In fact, problems for the deductivist arise as soon as we also recognise that (i) most event regularities of the causal sequence sort regarded as of interest to natural scientists are actually restricted to conditions of experimental control, whilst (ii) the results of these experiments are frequently successfully applied outside the experiments *where event regularities are not in evidence.*

The key to understanding this situation is already in place in the preceding discussion of the implicit ontology of economists' methods of mathematical-deductivist modelling. For the latter methods presuppose occurrences of event regularities of the causal sequence sort. And we have seen that in order to guarantee relevant results taking this *form*, economists need to specify their theories in terms of entities which are both isolated and produce constant and invariable responses to given conditions.

This analysis bears on how we must interpret experimentally produced event regularities. For we can make sense of the confinement of these regularities to experimental conditions just by viewing experimental practitioners as intervening in reality and experimentally manipulating it in order that (i) the workings of a specific intrinsically stable causal mechanism are (ii) insulated from the effects of countervailing factors. It is just because an intrinsically stable mechanism is so isolated, where it is, that an event regularity is produced between the triggering conditions of the mechanism and the effects that ensue. If a mechanism being investigated was not stable, or if countervailing factors were allowed to intervene, the regularity would not be produced.

Notice, then, that to make sense of the experimental process, it is essential to recognise that the event regularity produced corresponds to the empirical identification of an underlying causal mechanism. In other words, even in experimental work, i.e., even in scientific work which is most bound up with the production of event regularities of the sort under consideration, the primary concern is not with the production of an event regularity *per se*, but with the empirical identification of an underlying mechanism (co-responsible for any regularity so produced).

Notice, too, that it is only by way of this understanding of the experimental process that we can make sense of the observation, noted above but not yet addressed, that experimental knowledge is somehow successfully applied outside the laboratory, even in conditions where event regularities do not occur. For the knowledge or insights obtained relate primarily not to the (contingent and experiment-bound) regularity that is produced, but to a (experimentally empirically identified) mechanism that (when triggered) operates independently of scientists and their experimental work. For causal mechanisms normally act not actualistically (resulting in the same actual events or outcomes in all conditions), but transfactually (having effects all the time whatever the outcome). Thus gravitational mechanisms or tendencies will be acting on the autumn leaves not just as they fall to the ground but even as they fly over roof tops and chimneys.

There are many implications of this brief argument that could be developed (see Lawson, 1997a). But the central point I am wanting to convey here is that even in those experimental situations where event regularities are successfully produced the real contribution of (successful) science is not the production of the event regularity *per se*, but the identification of an underlying causal factor. The aim of experimental practices is to increase our understanding (or to `test' theories about) underlying powers, mechanisms and/or tendencies, etc., responsible for the events we produce or otherwise observe.

We find, then, that even the achievements of laboratory experimentation ultimately constitute evidence supporting the view that *if* anything is essential to the scientific process it is this movement from a surface phenomenon to its underlying cause. This is causal explanation (rather than event prediction). Now, the identification of causes is not restricted to situations where stable event regularities are produced. As I show elsewhere (e.g., Lawson, 1997a, 2003a) causes can be uncovered in situations where mathematical-deductivist reasoning is not applicable at all. The point of relevance here, though, is that even the experimental work experimental work of science is found to be concerned with the understanding of causal factors.

In short, *if* science can be characterised by any one aspect of its activities, the analysis sustained here and elsewhere (Lawson, 1997a) suggests that the prime candidate is the (explanatory) move from a conception of a phenomenon of interest at one level to a conception of its cause(s) lying at a different one. Science is characterised by causal explanation if by any one aspect or process. And, if the social ontology sketched above, and defended elsewhere (Lawson, 1997, 2003a) is at all correct, this is a move as much available to those who study social phenomena as to those who study natural phenomena. Economists can seek to uncover, for example, the social processes governing unemployment, poverty or whatever.

It follows that even if the practice of applying methods of mathematical-deductivist modelling in economics continues to be rather unsuccessful, there remains every reason to suppose that economists can yet, and successfully, practice science in the sense of (successful) natural science.

Implications of ontology

I stress or rather re-emphasise that the arguments so far made are about dangers and possibilities; they are not stipulative of how research must be done. I have not argued, for example, that formalistic methods have no place in economics and nor would I want to. I have suggested that their fruitfulness presupposes conditions that are found to be rather rare in the social realm, so that the practice of repeatedly insisting upon them is risky at best, and in the face of the subject's continuous poor showing, somewhat foolhardy. Further the arguments sustained indicate that proceeding in a manner that is explanatory successful and consistent with the practices of successful natural science remains a real possibility. But only the practices of substantive economics, not methodology, can actualise this possibility. Ontology as under-labouring, though, has clearly made a difference, not least in casting significant doubt on the validity of the usual reasons given for insisting on one (the mathematical deductive) approach.

Numerous more specific implications can be drawn, some being more constructive than others. On the critical front we can see, for example, that various attempts to defend the mainstream formalistic emphasis are misguided. For example I am aware of endeavour to establish the relevance of modern mainstream economics that actually proceeds by determining the proportion of all articles in core or `flagship' journals that make reference to `empirical facts' or `draw' policy implications, and reporting that this proportion is reasonably high.

The perspective sketched above casts clear doubt on the coherence of any such exercise. For if economic data record phenomena generated within an open and highly internally-related social system, and mainstream economists uncritically insist on analysing them using methods which presuppose they record phenomena generated in systems that are closed and atomistic, any claims by these economists to be in touch with reality just because data are involved are not well founded. Indeed they merely reveal the level of misunderstanding involved. Similarly, if the whole framework of theoretical modelling is inevitably, and known to be, largely false, it is not obvious there need be any relevance or insight in policy conclusions drawn.

Turning to consequences that are more constructive it is clear, for example, that ontology can play an important clarifying role by providing a categorical grammar against which more substantive social theoretical conceptions and distinctions can sometimes be better understood. Thus, all social systems and collectivities can be recognised as ensembles of networked, internally-related, positions (in process) with associated rules and practices. This applies to the state, schools, hospitals, trade unions, the household, and so forth. Sub-distinctions can be made. A social system can be recognised as a structured process of interaction; an institution, as already noted, as a social system/structure (or even a form of behaviour) that is relatively enduring and perceived as such; a collectivity as an internally-related set of social positions along with their occupants, and so forth (see Lawson, 1997a, pp 165-6).

The basic categories elaborated also provide the framework for a theory of *situated rationality* (Lawson, 1997a, chapter 13; 1997c). Various real interests, as well as possibilities for action, depend upon the internally-related positions in which individuals are situated. Of course, we all stand in a large number of (evolving and relationally defined) positions (as parents, children, immigrants, indigenous, old, young, teachers, etc., etc.). Hence there exist possibilities of conflicting, as well as unrecognised, individual, in addition to collective or shared, (evolving) interests (and intentions).

This conception, then, also provides the basis for a meaningful a theory of distribution. In particular it allows an analysis of the determinants of resources to positions, as well as of positions to people.

More generally, a conception such as that sustained encourages and informs a reconsideration of the many categories of social theorising taken for granted in modern economics. The list includes not only the already noted categories of institutions, systems, rationality, but also other equally central to economics such as money, markets, uncertainty, order and numerous others (see appendix below).

Also, by examining a contributor's ontological preconceptions it is often possible to throw further light on the nature and/or meanings of their substantive claims and contributions, especially where the latter are found to be otherwise open to a large number of seemingly ill-grounded interpretations[1]. And so on.

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[1] For example, through examining the relevant author's ontological preconceptions it has proven possible to give support to (contested) assessments that Commons did hold a theoretical perspective (see Lawson, C., 1994, 1995, 1996, 1999b); that Hayek's position changed significantly over time (Lawson, 1994; Fleetwood, 1995); that Keynes' rejection of econometrics was not a superficial response based on ignorance of the topic (Lawson, 1997d); that Veblen did favour an evolutionary economics and not merely because making economics evolutionary would render it up-to-date (Lawson, 2003a); that neither Smith nor even Newton adopted `Newtonian' methodology, and Smith's contribution is hardly in the mould of, or a precursor of,

Directionality

Continuing the constructive emphasis, let me finally consider some of the numerous ways a conception of ontology, and in particular the conception defended here, may impart directionality to social research.

Most clearly, and as I have already in effect suggested, because the social world is found to be structured (it is irreducible to such actualities as events and practices) it follows that *actualism* is a mistake, that social research will need to concern itself not only with correlating, or otherwise describing, surface actualities, but also, and seemingly primarily, with identifying the latter's underlying conditions. In other words, social research, like natural scientific research, has, as a compelling objective, the explaining of surface phenomena in terms of their underlying conditions. If patterns in surface social phenomena have scientific value it is in some part through their providing access to the structural conditions in virtue of which the former are possible. Of course, structural conditions in turn have their own conditions, so that the process of seeking to explain phenomena at one level in terms of causes at a deeper one may be without limit.

So the ontological conception sketched above, if sound, directs us towards considering how, in economics, we might conduct causal explanatory projects. This emphasis, in turn, points to a need to develop modes of inference over and above (the usual forms of) deductive and inductive logic. A reliance on these latter forms of reasoning, as usually interpreted, restricts the researcher to considering only the level of reality at which the phenomenon to be explained is found. However, for causal explanation it is usually necessary to go deeper. Deduction, of course, moves from the general statement to the particular. For example, if we accept that "all metals expand when heated" we can *deduce* that "this metal before us will expand when heated". Induction takes us from the particular statement to the general. If our research practices reveal that "each examined (bit of) metal expands when heated" we might be tempted to speculate *inductively* that "all metals expand when heated". In each case we move from a statement about the behaviour of metals to a second statement at the level of the behaviour of metals.

To pursue causal explanation as interpreted here, we require a mode of inference that takes us behind the surface phenomenon to its causes, or more generally from phenomena lying at one level to causes often lying at a different deeper one. This is *retroduction*. It takes us from a recognition that "this metal before us expands when heated" to a conception of the metal's intrinsic structure (or whatever) in virtue of which the metal has the power to expand when heated.

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general equilibrium theory (Montes, 2002, 2003); that Popper was ultimately not a `Popperian' (Runde, 1996); that Marx's theory (of capitalist tendencies) is not a deterministic theory (Brown, et al., 2002; Collier, 1989), and so on.

Little can be said outside a specific explanatory context about how in practice the retroductive process might proceed, other than it will often be helped along by a logic of analogy and/or metaphor, and rest usually upon ingenuity as well as luck (see *Economics and Reality* especially chapter 15). For example, following the discovery, in the late 1980s, that cows in the UK showed symptoms of the illness we now call `mad cows disease', it was retroduced, by way of analogy with other illnesses, that a virus was causing the problem. This retroduced hypothesis, however, proved not to be correct. Only with a lot of skill and luck was the prion located as the most likely (explanatory powerful) causal hypothesis (see for example Lawson 1997a, pp. 293-4). Still the need to be abreast of these additional forms of logical reasoning is an insight of the ontological reasoning above described.

Other insights can be briefly mentioned. For example, to the extent that social phenomena not only depend upon transformative human agency and so are processual but also are highly internally related, it is *prima facie* rather unlikely they are manipulable in any useful or meaningful way by experimental researchers and others. Social research, in consequence, will typically need to be backward looking, being concerned to render intelligible what has already occurred, rather than interventionist/experimentalist and so predictionist.

Further, it is easy to see that an ontological conception such as elaborated above can carry implications for matters of ethics and so for projects of a practical or policy sort. For example, because all human beings are both shaped by the evolving relations (to others) in which they stand as well as being differently (or uniquely) positioned, it follows that all actions, because they are potentially other-affecting, bear a moral aspect. Further, any policy programmes formulated without attention to differences, that presume homogeneity of human populations, are likely to be question begging from the outset. Certainly, programmes of action that ignore their likely impact on the wider community are immediately seen as potentially deficient. Eventually, of course, such considerations point to questions of power, democracy and legitimacy. They raise questions of who should be taking decisions in a world of different identities where most of us are likely in some way (differentially) affected by actions taken by others. And indeed they invite a questioning of whether anything less than the whole of humanity (and possibly much more) can constitute a relevant unit of focus in the shaping of emancipatory projects and actions.

Some final qualificatory remarks

Let me finish by sounding some notes of caution. I have argued that it makes sense to treat the contribution of at least some methodologists or philosophers to economics as engaged in activities of under-labouring. The aim is to aid, not to supplant or to instruct (or to make no difference at all to), the economic theorist. But I do not want to suggest that all methodologists always approach their task in this ‘mean’ spirit. Many, I accept, presume to achieve more (or nothing of critical consequence at all). But it does not follow that all do. And the realist project emanating from Cambridge over the last sixteen years or so in particular has been concerned with under-labouring, especially in the context of economics.

Second, there is nothing to prevent those who contribute to such an under-labouring project also being involved in substantive theorising and policy analysis. But it does mean that such activities must be distinguished from those of philosophy. This applies as much to the results of realist philosophy as to those of any other. Any derivation of substantive theoretical results, reliance on specific methods and/or support for concrete policy proposals, requires that the ontological conception sustained be augmented by specific empirical claims, as I have often stressed.

It is quite legitimate (and not uncommon) for those accepting the broad framework of critical realism to disagree over additional empirical claims, with different individual contributors thus arriving at contrasting substantive, methodological or political orientations for specific contexts (see Clive Lawson, et al., 1996). The point is that although critical realism makes a difference to the sorts of approaches or frameworks adopted and so paths taken, it is never by itself determining of substantive positions reached. There is not a position on substantive theory, policy, or practice, even in a particular context, that warrants being distinguished as *the* realist position (see Lawson, 1996, pp. 417-9).

Finally, I must emphasise that an under-labouring contribution, including one such as described above which concentrates on ontology, is (just like any other type of contribution to knowledge) inevitably fallible and partial and, in some aspects at least, doubtless transient. And I stress that I do not argue for any prioritising of the role of philosophy, even of ontology. I do think explicit and sustained ontological analysis, or its results, can be invaluable, and at this juncture, given the state of the modern discipline, probably essential. But as I say, ontology is itself a situated, limited, fallible (and of course always culturally conditioned) process, producing results that are likely to be transient, at least partially.

I thus urge a rounded approach to theorising in economics. I advocate only that developments in ontology and those in method and substantive theorising evolve in tandem, with each informing or otherwise enriching the others, where possible.

A division of labour is vital. There is plenty of scope for highly differentiated research; variety, as always, is fundamental. In emphasising the need and worth of philosophy in its under-labourer capacity I am, I suppose, revealing my own meanest of dispositions. But I readily accept there is point to such mean endeavour only as long as there is simultaneously a wider or larger concern with the pursuit of economics as substantive science. The objective, indeed, is in some part to contribute to clearing the ground so that a few more specifically *economist* scientists might at least stand a chance of (exploring ways of) producing `mighty designs' that `leave lasting monuments to the admiration of posterity'.

Appendix: Further Reading

Many of the central contributors to economics, including Smith, Mill, Marx, Marshall, Veblen, Keynes and Hayek, were explicitly philosophical/methodological in their orientation (see for example Dan Hausman's *The Philosophy of Economics: An Anthology*). However, by the middle of the twentieth century, explicitly methodological texts had become rather rare, particularly where the project of mathematising economics was becoming dominant. Perhaps the most widely known explicitly methodological later work was Milton Friedman's (1953) essay; a contribution that basically attempts to justify the unrealistic nature of mainstream economics. In 1980 Mark Blaug's *The Methodology of Economics: Or How Economists Explain* appeared, and was noticeable not least for being one of the very few books devoted to the topic of economic methodology. Since then matters have improved. The *Journal of Economic Methodology* and *Economics and Philosophy* have been established, and heterodox economists at least can be found writing books and chapters of a methodological kind.

In the last twenty years, as economic methodology has gradually grown in importance, there has been a move away from methodology that is prescriptive in any simplistic way, such as laying down rules of analysis that only have to be followed. Much of the new methodology is surveyed in Wade Hand's (2001) aptly titled *Reflection Without Rules* as well as Sheila Dow's (2002) *Economic Methodology: an inquiry*.

The topic of the current chapter of course has been philosophical under-labouring in the form of ontology (for an early discussion of philosophy as under-labourer in the economics context, see Clive Lawson, Peacock and Pratten, 1996). Several books on social ontology, focussing on economics in particular, have recently appeared. These include Steve Fleetwood's (1999) collection *Critical Realism in Economics: Development and Debate*; my own *Economics and Reality* (Lawson, 1997) and *Reorienting Economics* (Lawson, 2003); Uskali Mäki's (2001) edited collection *The Economic World View;* as well as the volume edited by Paul Lewis (2004) titled *Transforming Economics: Critical Realism as Others See It.*

Most of the latter texts deal primarily with philosophical ontology, that is, with seeking to determine the general or shared properties of social phenomena. But I should finally note that are also numerous contributions to scientific ontology, that is, with studying the nature of specific social phenomena or entities. The list of entities recently explored includes money (Ingham, 1996, 2004), the firm and region (Lawson, C., 1999a, 2002), institutions (Lawson, 1997a); transactions (Pratten, 1997), the individual (Davis, 2003, 2004) social order (Fleetwood, 1995, 1996), collective learning (Lawson, C., 2000), causality (Fleetwood, 2001; open and closed systems (Bigo, forthcoming); Lewis, 2000a; Runde, 1998a), tendencies, (Pratten, 1998; Lawson, 1989, 1997a, 1998), markets (O'Neil, 1998), households (Ruwanpura, 2002), consciousness (Faulkner, 2002); uncertainty (Dunn, 2000, 2001); macroeconomics (Smithin, 2004), space (Sayer, 2000), probabilities (Runde, 1996, 1998b, 2001), trust (Reed, 2001), technology (Lawson, C., forthcoming), metaphor (Lewis, 1996, 2000b).

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