Really Reorienting Modern Economics[1]

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Modern economics can benefit significantly both from a radical reorientation at the level of method, as well as from a greater input from appropriate branches of philosophy. This is the two part (albeit highly interrelated) thesis I want briefly to defend in this short paper.

The economic crisis has drawn increased attention to the explanatory failures of modern economics; and a good number of commentators are now questioning how the discipline can do better. I have long argued that many of the various problems of the modern discipline derive from the heavy *emphasis* on formalistic modelling. Though there are other economists taking a similar view, it is noticeable that most of the ongoing response from the economics academy takes the form of proposals either for revised mathematical deductive 'models', or for revised *approaches* to mathematical deductive 'modelling'. Such reactions are doubtless of value. However, if this is the only sort of redress fostered by the recently formed *Institute for New Economic Thinking* then I fear its name will be found to be somewhat premature. In any case, I intend to argue that a rather more radical reorientation of the discipline is required.

It is often supposed that any oppositional orientation to the modern emphasis on mathematical method in economics is based on personal preference (distaste) or (limited) analytical competence. It is rarely recognised (or acknowledged) that such an opposition may be an informed choice; that there may actually be good reason to doubt the generalised appropriateness to economic analysis of the sorts of mathematical methods mostly employed by economists, *given the nature of social phenomena*.

This though is the contention I intend to advance explicitly here. The sorts of mathematical methods economists use are forms of tools. Like all tools they are appropriate to some uses and conditions and not to others. Though a hammer has various uses it is not particularly relevant to cutting the grass. I want to suggest that mathematical methods of the sort economists typically employ may not be particularly, or very often, well-suited for the illumination of social material, given the nature of the latter. The reasonable way forward, I thus argue, is explicitly to design explanatory approaches to be appropriate to the sorts of contexts and materials with which economists must actually deal, even if this means relinquishing the current formalistic emphasis. A philosophical input can help in this latter endeavour.

[1] This paper develops arguments originally published in Lawson 2009b, and 2009c.

A concern with investigating the nature or structure of material being studied is a practice that belongs to that branch of philosophy called ontology. This is a form of investigation rarely explicitly, let alone systematically, undertaken in modern economics, at least outside heterodox contributions. However, it is not so uncommon in the other fields of social science; nor is it entirely absent from contributions of some very influential economists in the early or middle parts of the preceding century (see especially writers such as Keynes[2] and Hayek[3]). Its neglect in much of modern economics, I believe, is a factor allowing the current worrying state of the discipline to prevail; indeed I am of the view that something along the lines of establishing *centres for the study of ontology* in economics would contribute significantly to facilitating a more explanatorily fruitful discipline.

Here my concern is specifically with *social* ontology, that is, with the study of the nature of those phenomena whose existence necessarily depends at least in part on us. Actually my focus is on studying, in combination, *both* the nature of social phenomena, and the ontological preconditions of the sorts of mathematical deductive methods that economists employ (i.e., the conditions under which the latter methods have a chance of providing insight). With this double focus it will be seen that there is a mismatch between the nature of social material and the conditions required for mathematical methods to have utility, a mismatch that helps explain the continuing numerous failures of the formalist project in modern economics.

I stress, though, that the results obtained through ontological elaboration are not entirely, or even mainly, negative; as I have already noted, insights achieved into the nature of social material also serve to indicate the sorts of conditions to which research methods ought to be tailored if the successful illumination of social reality is to be possible at least in principle. (They also allow us to address in an informed manner a host of additional interesting and relevant methodological and other issues of a sort that more critical commentators often raise[4]).

Nor even (to anticipate something of my findings) do I contend that the application of mathematical reasoning *could never* be useful to social theorising. There may well be local

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[2] Keynes' *A Treatise on Probability* (1973a), for example, is essentially an ontological investigation of the sorts of conditions under which inductive arguments in general are valid and judgements of probability specifically are legitimate (see for example Lawson 2003a).

[3] Hayek's "Scientism and the Study of Society" reprinted in the *Counter-Revolution of Science*, for example, is essentially an ontological questioning of the nature of social material and an argument that the peculiarity of the latter points to the need for methods other than those prevalent in the natural sciences (see Lawson 1996, Lawson 1997, chapter 10)

[4] I am thinking of questions concerns like the limits of social analysis; whether a (Darwinian) evolutionary economics is feasible; whether a non-mathematical economics can be scientific; what ethical stances are sustainable; what are the nature of the different heterodox traditions; what even is the nature of economics and what is its relation to the other branches of social science; whether indeed the current separation of economics from the other branches of social science is justified; the justification and basis (if any) of cross, post, or multi disciplinary research, and so forth. These questions and more are addressed in various places, perhaps most especially in Lawson 2003a.

scenarios, for example, in which the conditions of relevance of formalistic approaches show up. The analysis that follows (along with that which is found elsewhere – see e.g., Lawson, 1997, 2003a) gives grounding to criticism not of specific justified formalistic exercises, or of trial and error experimentation using formalism in economics, but of the almost exclusive *emphasis* on formalism found throughout the modern economics academy. Lectures, research papers, appointments, promotions, prizes, prestige and the rest of it, almost always turn on an insistence that formalism is the defining feature of the modern discipline, the proper way to do economics. The latter *emphasis* I believe is *the* fundamental problem of the modern discipline of economics.

As a final introductory comment I might emphasise that any criticism of the current emphasis on formalism that is advanced below is offered in the spirit of a pro-, rather than an anti-, mathematics stance[5]. The target of criticism is (of course) not formalism *per se* or its use in conditions where it has a chance of being appropriate; but its misuse, and, I repeat, specifically its usage in conditions in which it actually hinders the production of insight[6]. Unfortunately, I believe the latter situation characterises much of modern economics.

Because the suggested (ontological) mismatch of mathematical methods and the nature of social phenomena is, if am I correct about it, the more significant issue for understanding the course of the discipline and any future successes at social illumination, I will consider it first below before turning explicitly to the explicit themes of the session for which this paper has been invited/prepared, namely whether mathematical models are “rigorously testable, qualitative metaphors, or simply an entry barrier”. The arguments I make in this first part will in any case have a bearing on anything I have to say on the latter.

Some relevant background

In proposing a significant reorientation of the discipline, I am obviously taking it as given that currently there is something wrong with modern academic economics, and that this situation is now very widely recognised. But I am also taking it to be the case that there has been something wrong with academic economics for a considerable period of time, and this has long been widely appreciated as well. Not only have economic contributions long been peppered with conceptions widely accepted as fictitious (rational expectations, representative agents, two commodity worlds, human super-calculators, etc., etc.) but they also over a

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[5] Perhaps it is not inappropriate if I mention here that my training is in mathematics, and the latter is a subject I continue to enjoy.

[6] My perception of the use of formalism in modern economics is that, very often at least, it is somewhat analogous to the violin being used as a drumstick. If it is clear that criticism of the latter practice does not presuppose a dislike of violin music then it is hopefully equally apparent that criticism of the use of formalism in modern economics need not imply an anti-mathematics orientation. In most cases I believe such criticism reveals the opposite orientation, and involves a recognition of the proper limits of instruments as tools whether for producing music or for social illumination.

considerable period of time been found (and reported) to be in explanatory (and not just predictionist) terms ultimately rather unsuccessful[7].

I recognise, though, that some prominent contributors do, on occasion at least, seem to convey a somewhat different impression. For example, the opening two sentences of a recent popular contribution by Paul Krugman (2009) run as follows:

> "It's hard to believe now, but not long ago economists were congratulating themselves over the success of their field. Those successes — or so they believed — were both theoretical and practical, leading to a golden era for the profession".

Though prominently placed and so widely observed, I believe this assessment is actually rather far from capturing the truth of the situation. Or at least this is so if, by reference to the category "economists", we understand the full range of academics and others occupied with studying the economy[8]. Of course, much of the long term worry about the state of the discipline has been voiced by economists frequently styled or classified as heterodox[9]; or by others outside the economics academy[10]; including some outside the academy altogether[11].

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[7] (see Lawson, 2003a chapter 1)

[8] Certainly, in writing my own books my assessment has led me to use the opening lines such as the following:

> "Contemporary academic economics is not in a particularly healthy state. Over many years now problems have regularly come to light which throw considerable doubt on the capacity of many of its strands to explain, or even always to address, real world events or to facilitate policy evaluation. Such problems especially beset the rather dominant `mainstream' or `orthodox' project, centring on econometrics and formalistic `economic theory', which is my main concern here. This unhappy situation, moreover, appears to be increasingly recognised both inside and outside of the academy" (Lawson, 1997, p. 3)

[9] For example by Austrian economists, feminist economists, (old) institutionalists, Marxian economists, post Keynesians, social economists and numerous others (see e.g., Lawson 2006).

[10] Thus Richard Parker writes, on assessing the overall state of the discipline:

> "[E]conomists no longer agree about what they do, or even whether it is all worth doing. Critics outside the profession long faulted economists for a host of sins: their deductive method, their formalism, their over-reliance on arcane algebra, their imperviousness to complex evidence, the bald inconsistency of different facets of the economic paradigm. What's new--after decades of steadfast resistance--is that these same concerns have begun to bother the profession too" (Parker, 1993, p. 1).

[11] For example, in an award winning book the former British minister Lord Howell (in a chapter entitled "The failure of modern economics to explain what is going on") assesses the sate of modern economics as follows:

> "The paradox of modern economics is that while the computers are churning out more and more figures, giving more and more spurious precision to economic pronouncements, the assumptions behind this fiesta of quantification are looking less and less safe. Economic model making was never easier to undertake and never more disconnected from reality.
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> Somewhere along the way economics took a wrong turn. What has occurred, and what been vastly accentuated by the information revolution and its impact, is that economists have drained economic analysis both out of philosophy and out of real life, and have produced an abstract monstrosity, a world of models and assumptions increasingly disconnected from everyday experience and from discernible patterns of human behaviour, whether at the individual or the institutional level.
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> As a result, economists have not only failed to discern, explain or predict most of the ills which beset the world economy and society, but they have actively encouraged a deformity of perception amongst policy makers and communicators, which has led in turn to a deep public bewilderment and distrust of government authorities - and this at the very time when the need is greater than ever for a bond of trust between government and society.

But certain influential mainstream economists have been equally aware enough to express similar views. For example in a speech acknowledging the award of the Nobel Memorial Prize to John Nash, the game theorist Ariel Rubinstein writes:

> "The issue of interpreting economic theory is... the most serious problem now facing economic theorists. The feeling among many of us can be summarized as follows. Economic theory should deal with the real world. It is not a branch of abstract mathematics even though it utilises abstract tools. Since it is about the real world, people expect the theory to prove useful in achieving practical goals. But economic theory has not delivered the goods. Predictions from economic theory are not nearly as accurate as those by the natural sciences, and the link between economic theory and practical problems... is tenuous at best" (Rubinstein, 1995, p. 12).

And this mainstream `theorist' continues:

> "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).

In fact, given the way Krugman divides the macroeconomics community into two factions, *saltwater* economists ("mainly in coastal US universities") and *freshwater* economists ("mainly in inland schools") it is easy to get the impression that his category of economists excludes not only all except those that can be regarded as mainstream but everyone positioned outside the United States. However, even if restricted to the views of mainstream economists located in North America, Kugman's assertions are misleading. Thus we find the past Nobel Memorial Prize winner Wassily Leontief observing in the early 1980s that:

> "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" (Leontief, 1982, p. 104).

At the end of the 1990s Milton Friedman, also a Nobel Memorial Prize winner, echoes similar sentiments:

> "economics has become increasingly an arcane branch of mathematics rather than dealing with real economic problems" (Friedman, 1999, p. 137).

And Ronald Coase, yet another Nobel Memorial Prize winner, remarks that:

This misleading `black box' view of the world purveyed by the economics profession (with heroic exceptions), at all levels from the most intimate micro workings of markets to the macro level of nation states and their jurisdictions , has been vastly reinforced by compliant statisticians who have brought a spurious precision and quantification to entities and concepts which may not in fact have any existence outside economic theory, or whose validity has been sapped away by the impact of information technology" (Howell, 2000, chapter 5).

> "Existing economics is a theoretical system which floats in the air and which bears little relation to what happens in the real world" (Coase, 1999, p. 2)

Nor is econometrics spared. Thus the leading North American econometrician, Edward Leamer, has observed that after "three decades of churning out estimates the econometrics club finds itself under critical scrutiny and faces incredulity as never before" (1983, p. 42), and goes as far to suggest that "hardly anyone takes anyone else's data analysis seriously" (p. 37). He earlier observed that:

> "The opinion that econometric theory is largely irrelevant is held by an embarrassingly large share of the economics profession. The wide gap between econometric theory and econometric practice might be expected to cause professional tension. In fact, a calm equilibrium permeates our journals and our meetings. We comfortably divide ourselves into a celibate priesthood of statistical theorists, on the one hand, and a legion of inveterate sinner-data analysts, on the other. The priests are empowered to draw up lists of sins and are revered for the special talents they display. Sinners are not expected to avoid sins; they need only confess their errors openly" (Leamer, 1978, p. vi).

Summing up the whole situation, Mark Blaug, who long sought to defend practices of modern economics, came to conclude:

> "Modern economics is sick. Economics has increasingly become an intellectual game played for its own sake and not for its practical consequences for understanding the economic world. Economists have converted the subject into a sort of social mathematics in which analytical rigour is everything and practical relevance is nothing" (Blaug, 1997, p. 3)

I run through all this merely to emphasise that, contrary to the assertions of Paul Krugman, a recognition of the sorry state of economics, and least for many of a rather more critical disposition, is not a recent phenomenon, one that emerged with the onset of the recent crisis; rather it is a situation that is relatively longstanding. Moreover, most of the observations recorded above point to worries about the efficacy of mathematical models or modelling practices. This background situation is my starting point.

The fundamental mismatch of modern economics

As already noted, an obvious, and the most apparent, response to the noted state of affairs is for economists to suggest that the cause of the problems lies in the nature of *specific* formalistic models and/or modelling practices, and so to advance mathematical alternatives (see discussion in Colander *et al*, 2008, 2009; Lawson, 2009b, 2009c). My alternative contention is that the central problem derives from a reliance upon methods of mathematical deductive modelling *per se*; that there is a basic mismatch between the sorts of mathematical methods economists employ and the nature of the social, including economic, phenomena that economists seek to illuminate.

Here, then, I turn to social ontology. Consider first the conditions for which the sorts of mathematical deductive methods that economists use will have utility. Most fundamentally these methods can be seen to be restricted in their applicability to closed systems, meaning those in which event regularities or correlations occur. Many recognise that to date such closures have been found to occur only very rarely in the social realm. I want to suggest that we also we have good reason to suppose they will remain uncommon.

In fact, closures are relatively uncommon even in the natural sciences. As it happens, outside astronomy, most of the event regularities known to natural science occur in conditions of controlled laboratory experimentation – or experimental closures. They arise when an experimenter succeeds in isolating/insulating an intrinsically stable mechanism from the effects of countervailing factors. Under such conditions a regularity can be produced correlating the triggering of the mechanism with its unimpeded effects.

Two conditions for guaranteeing a closure are apparent in this experimental case. The first is that we are dealing with a mechanism that is intrinsically constant. The second is that a situation can be engineered for ensuring that this mechanism, if triggered, acts in relative isolation. We can refer to these two conditions respectively as the *intrinsic and extrinsic closure conditions*.

Although, other, perhaps very different sets of sufficiency conditions are possible in principle, it is difficult to imagine what they might be in practice; and more to the point it is these two conditions – the intrinsic and extrinsic closure conditions - that modern economists mostly, if implicitly, seek to satisfy in their theorising around their economic models.

Of course whereas experimental natural scientists typically[12] work laboriously to achieve the (experimental) isolation of a relevant mechanism, economic modellers heroically assume that such isolations of intrinsically constant causal factors occur quite spontaneously in the social realm, and indeed are even ubiquitous.

[12] The exception of course is the celestial closure underpinning movements of planets. It is important to recognise however that although the celestial example is spectacular in nature, it represents a relative rarity in constituting a spontaneous (demi-) regularity of its sort. No doubt it is precisely its spectacular nature that accounts in some part for the general failure from Laplace onwards to realise that the situation *is* relatively uncommon, to appreciate that the celestial pattern, or near closure supporting it, is far from being indicative of the phenomenal situation that can be expected to prevail more or less everywhere. This failure, in turn, appears to be largely responsible for the widespread, if tacit, acceptance, formerly in philosophy, and currently in the social sciences in particular, of a ubiquity of constant conjunctions of events in nature, and thus of the doctrine of the actuality of 'causal' laws (that they precisely take the form of event regularities). It no doubt also encourages the idea that methods of mathematical deductive reasoning have ubiquitous relevance, when in fact conditions under which they are relevant, certainly as they are formulated in modern economics, appear to be circumscribed indeed.

The nature of social phenomena

However, it is easy enough to see that the constitution of social reality is such that, by and large, the two identified conditions for a closure are unlikely very often to be satisfied in this domain.

Consider the extrinsic condition for a closure first. Instead of existing in isolation almost all social phenomena are in fact constituted in relation to each other. It is easy enough in modern capitalism to recognise the internal relationality[13] of markets and money and of firms and governments and households, etc; all depend on and presuppose, indeed are constituted through, each other. It would be futile to seek (experimentally or otherwise) to isolate any one from the influence of the others; just as it would be analytically pointless to treat any as if such isolation were the case.

And human individuals as social beings are likewise formed in relation to others. All slot into positions, where all positions are constituted in relation to other positions. Thus employer and employee presuppose each other, as do teacher and student, landlord/lady and tenant, parent and child, gendered man and woman, and so on. We all slot into, and are moulded through the occupancy of, a multitude of such positions, deriving real interests from them, and drawing upon whatever powers or rights and obligations are associated with those positions[14].

So social reality is an interdependent, network, it is an internally related totality of rights and obligations and so forth, not a set of phenomena each existing in relative isolation.

Nor does the hope of satisfying the intrinsic condition for a closure seem any more promising. For everything social (whose existence depends necessarily on us) is constantly being transformed through human practice. Think of a language such as English. At any point in time it exists as a (largely unacknowledged) resource that is drawn upon in our speech acts and so forth. But through the sum total of the practices all those people who at any point in time are speaking, writing and reading English, the language is (largely unintentionally) being reproduced and, in part, transformed. It thus exists as a process, as something that is constantly being reproduced and transformed through practice. This is its mode of being; it is intrinsically dynamic and subject to transformation. But a moment's reflection reveals that all social phenomena share this mode of being: universities, towns, pollution, financial centres, financial instruments, each and every social organisation, all

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[13] *Internal relations* are relations whereby the aspects related, the relata, just are what they are, and/or are able to do what they do, in virtue of the relation in which they stand. Internal relations hold for the natural world too, e.g., between a magnet and its field. Notice, though, that it is relations between positions (as opposed to people *per se*) that are likely to be of primary importance in the social domain. (*External relations* hold where the relata are constituted independently of the relation. For example that between the barking dog and the post person.

[14] Lengthy arguments in support of these assessments can be found for example in Lawson 2003 (see especially chapter 2)

aspects of the economy, society at large, our positions and their associated powers, our social identities, embodied personalities and everything else whose existence necessarily depends at least in part on us. So a satisfaction of the intrinsic condition for a closure again is something not to be taken for granted.

It should be clear, though, that it would be inappropriate to assert in advance that the satisfaction of the intrinsic and extrinsic closure conditions, facilitating the use of formal models, could never be the case. Sometimes certain fairly fixed mechanisms may so dominate others that they operate almost as though acting in isolation. Traffic behaviour in rush hours in major cities may sometimes be like this, as may the demand for heating at certain hours of the day in very cold climates. Clearly, these are issues to be investigated.

But most fundamentally here, the satisfaction of the closure conditions is something that cannot either be taken for granted; indeed there is good reason to suppose that the satisfaction of these two conditions is an uncommon event in the social realm.

Of course social reality is more complex still. It contains meaning and value and so forth. But already enough has been said to account for the general empirical failings of much of modern academic economics with its emphasis on mathematical modelling (as well as its employment of bizarre assumptions such as rational expectations, representative agents, two commodity worlds and all the rest of it). These failings, and others, arise as the result of the constant endeavour to present phenomena of social reality that are mostly really open, relational and processual as if they are everywhere closed, intrinsically constant and effectively isolated or insulated from each other.

The way forward

So what is to be done? Implications clearly follow for reorienting (the practices of) the economics academy. A pertinent question here is whether the conception of the nature of social reality sketched above not only underpins a critique of the mainstream emphasis on formalistic modelling, but also points a way to, doing economics differently, as well as (given the interest of the current forum) to understanding the nature of the financial crisis.

It does indeed point to an alternative explanatory approach, one that can be systematised as contrast explanation. I have explored and developed this explanatory strategy at length elsewhere (see for example Lawson, 1997, chapter 15; 2003a chapter 4; 2009a; or various chapters in Fullbrook, 2009). However, I suspect that in the forum for which the current paper is prepared, the dominant interest will be on ways of gaining understanding of the economic crisis. A major contribution of the above ontological sketch in this regard, I think, lies in pointing to an appropriate framework of analysis. Let me explore this contention a little.

I have suggested that social reality is an open, structured, totality in motion. It is a dynamic totality in which we all occupy positions that bind us to others through a network of rights and obligations.

This totality includes the financial system and anything we might want to call the economy. Within the network of accepted social positions and associated rights and obligations that coordinate social life, has arisen over time a measuring and accounting system bound up with numerous devices some of which we call money. The system that has evolved allows a subset of obligations and rights to emerge and proliferate taking the form of credit and debit. Alternatively put, amongst the numerous social positions in which individuals (including legal individuals called companies) find themselves, and which bind them to others and the rest of society, are very often those of debtor and creditor, and numerous individuals are often positioned as both.

A debtor owes a debt to a creditor and thereby is usually under an obligation to the latter in the sense of being duty bound at some stage to provide the latter with something of value. As such, markers of this debt (forms of 'money' or whatever) become valuable in themselves, and many types can be traded or exchanged, thus effecting a transference of specific rights to credit.

Such a system is stable and indeed functional only if the debtors are, and are considered to be, reliable, both in the sense of being committed to, and capable of, fulfilling the obligations involved. The system is thus based on trust and confidence on the part of creditors, and on promises, good intentions (or trustworthiness) and material credibility on the part of debtors.

Expectations and placements of trust, though, are easily disappointed. There is nothing in this system that prevents the level of borrowing, the expansion of credit/debt, from getting way beyond levels at which debtors can meet their obligations. Both debtors and creditors can be over-optimistic about investment possibilities that exist. Or the situation can easily change so that earlier seemingly profitable opportunities and decisions are rendered otherwise. In numerous ways there can be an expansion of credit/debt way beyond levels that the system is found *a posteriori* to be able to sustain. This is the scenario of the last twenty five years or so, a period which has witnessed a massive expansion in credit/debt. When, in such a scenario, trust and confidence break down, we can have the sort of crisis such as we have recently witnessed.

Of course, the details of the recent period are complex, and a full understanding requires, amongst other things a detailed analysis of the numerous structural transformations in the financial sector during this period, as well as an exploration of the nature of mechanisms whereby an expansion of credit/debt has occurred (on this see Lawson, 2009b[15]); but this brief sketch does, I believe, indicate the relevance of the framework.

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[15] Indeed all the contributions to the symposium of the *Cambridge Journal of Economics* in which the latter paper (Lawson 2009b) appeared.

The latter being so, and given the basic nature of the structures and mechanisms involved, it is clear that the recent crisis situation (like almost any social situation) is something that needs to be understood rather than modelled.

At all points in, and stages of development of, the financial system, we are faced not with a ubiquity of regular behavioural patterns underpinned by isolated systems of human (or other) atoms, but with the perpetual emergence of novelty, not least at the level of relational structures, underpinning transformed mechanisms and practices. This sort of continual emergence within a relationally structured, interconnected, totality in motion, is seemingly the essence of any financial system within capitalism

Accepting the sort of framework I have begun to sketch above, it is apparent that the legitimate and feasible goal of economic analysis is not to attempt to mathematically model¸ perhaps even with the hope of predicting crises and such like, but to understand the ever emerging relational structures and mechanisms that render them more or less feasible or likely.

I do emphasise this. Even amongst heterodox economists there seems currently to be a kind of competition going on to see who best anticipated the crisis, including its timing. This is mostly beside the point. At the races there is always someone who bets on the winning horse, even when an outsider wins; but no one person always gets it right. A better analogy is with the construction of buildings in earthquake zones, or preparing for new virus epidemics. The goal of researchers in these fields is not to predict precise developments, but to seek an understanding of relevant systems, structures, mechanisms, processes and potentials that is sufficient to allow the production and placement of additional structures and mechanisms, where feasible, that help prevent the emergence of conditions of human disasters. The same sort of understanding can be sought in economics to help prevent the emergence of economic crises[16].

As already noted I have explored these sorts of issues elsewhere (see Lawson, 2009b). Though obviously relevant to any concern with transforming the economy, it is just as clear that they take me increasingly away from the specific theme of the session for which this paper is prepared. So let me put these concerns aside here and finally turn and address head on the specific topics or themes for which this paper was prepared.

[16] Amongst other things, the latter understanding requires an account of the background conditions against which ongoing developments are taking place. In the current context, this includes understanding how the credit expansion triggered by liberalised financial markets set the conditions for the current situation, and the assortment of developments and mechanisms by which it has come about.

Mathematical models: rigorously testable, qualitative metaphors, or simply an entry barrier

The topic of the session for which this talk is invited is whether mathematical models are “rigorously testable, qualitative metaphors, or simply an entry barrier”. I assume that this set of questions is posed in the current context just because it is anticipated that the answers bears somehow on the more fundamental one of whether a significant reliance upon mathematical deductive modelling is justified after all. Let me, then, consider the noted themes in turn, each through the lens of this particular concern.

Rigorous testability

With regard to the first theme of rigorous testability, I assume we are focussing on econometrics and econometric models. Is an econometric model testable? I think it typically is as a totality. That is, I mean that any test is never just of the economic content of a model, but always of all the numerous hypotheses and assumptions, including and hidden or unelaborated statistical ones (including regarding shapes of probability distributions, etc). The model is a package. Of course, in statistical hypothesis testing there is always a lot of interpretation as well as convention involved. But the rarely emphasised fact of the matter is that, given the theoretical standards or conventions that econometricians usually adopt, just about all models tested against data over the last fifty years or so have in effect been rejected. Often the results are presented or reported as though this is not so. But in most such cases, as many econometricians themselves seem often to acknowledge, so many regressions are run to achieve the (relatively few) results reported, that the conditions of the test are violated in the process, rendering the results more or less meaningless at best.

Of course, this does not preclude the possibility that some model will pass all the relevant tests in a legitimate way in due course. And much work is currently devoted to adapting models, modelling procedures and frameworks, and tests, and so forth. Of course I am supportive. Certainly I do not want to belittle such endeavour. In particular, I can easily understand the turn to analyses with panel data, or to time series analyses using the co-integrated vector auto regression (CVAR) approach. The latter does seem to take seriously the complexity of societal processes and any resulting non-stationarity of data, and puts the emphasis on discovery and ‘digging’ as much as verification, and doing so in a systematic and structured fashion (see for example, Juselius, 2007; Hoover et al, 2008; Framroze Møller, 2008).

However, all such approaches seem still to rely on the availability of enduring event regularities of some sort (or regularities in data patterns at some level), and, if advanced in the spirit of maintaining the current *emphasis* on formal modelling, implicitly at least, posit (or presuppose), a ubiquity of such event regularities.

To this point few significant event regularities have been turned up in the social realm. And above I have elaborated reasons to suspect that few may be uncovered, no matter how sophisticated and open-minded the orientation to data analyses that is adopted. As such it is surely fair to ask just how much failure in the process of uncovering significant (non-spurious and enduring) regularities is required before it is 1) acknowledged that the hypothesis of a ubiquity of event regularities might be false, and 2) accepted - really accepted - that non-formalistic approaches that do not posit such regularities can be included as legitimate *additional* ways of proceeding in the wider endeavour that is applied economics.

There are real alternatives in this regard as I have often argued (See e.g., Lawson, 2009a, 2003a, chapter 4). At least there are, once it is recognised that social reality is structured (there are causal mechanisms behind the events of experiences) and that a legitimate (and perhaps the central) goal of science is to uncover the causal mechanisms responsible for co-producing the phenomena of experience. We may not be able to predict the path of each falling leaf. But uncovering the gravitational, aerodynamic and thermal, etc., mechanisms that bear on that path enable us successfully to send rockets to the moon. And the same move, from surface phenomena to deeper causes, is just as feasible in the social realm (again see e.g., Lawson, 2009a).

Even in the social realm conceptions of causal mechanisms are open to relative evaluation in the light of relevant empirical observations. But the manner in which this is achieved is not always conducive to formal analysis, and draws typically on a wide range of observation reports found to be relevant according to the context of the analysis (Lawson, 2003a, chapter 4; 2009a).

In short, it seems that econometric models are testable in the familiar sense. But a feature that is remarkable is how few models have legitimately been found to withstand such testing. From a wider perspective the implicit hypothesis that is really being repeatedly tested over the years is that there is a ubiquity of significant social event regularities to be uncovered. And the majority of results of the econometrics project to date appear to cast doubt on this hypothesis.

In any case, if the issue is whether the current heavy reliance on mathematical deductive models in modern economics is justified just because of the apparent *testability* of econometric versions, I think the answer has to be 'not really'. Coherence with the real world, including the ability to *withstand* empirical testing would seem to matter too.

Qualitative metaphors

What about the idea that any reference to a model in modern economics is metaphorical? Specifically is reference to a mathematical model appropriately interpreted as metaphor, and if so does this somehow rescue the mathematising project of modern economics from the sorts of (ontological) criticisms I have sketched above?

What, first of all, is metaphor and how exactly does metaphor work? Metaphor, I take it, is a device that facilitates understanding and knowledge development, by way of making connections between two domains which hitherto may not have been recognised as having parallels. It does so, in effect, by way of revealing or suggesting that an object or feature in the source domain (the vehicle of the metaphor) and an object or feature in the target domain (the tenor) are both tokens of the same type, or each a concretisation of the same more abstract object[17] (see e.g. Boyd, 1993; Soskice, 1985; Soskice and Harré, 1982).

If, for example, we say that Jack is a sheep, we are suggesting there is a more general or abstract class of objects of which Jack and a sheep are both tokens or particular sub-types. In this example the class may be of all creatures disposed to following others rather easily. If we say that Jill is a parrot we may well be meaning to suggest that Jill, like the parrot, is a special case of creatures that repeat or mimic what they hear. If it is said that trading is stagnant the general class is presumably that of all things where movement or activity is feasible but hardly happening. If it is said that prices have reached their ceiling, the general class is presumably anything that has an upper limit and has reached it.

Metaphor works, then, by connecting objects or aspects previously regarded as unconnected, by showing them both to be special cases of the same general thing, to be tokens of the same type. In making this connection, metaphor can serve to highlight features of a target object by way of likening it to the object in the source domain. It allows us to set up a generic system, using insights from the source domain, which possesses the potential to provide lines of development in the target domain.

Is the practice of referring to a (set of) mathematical deductive formulation(s) as a model metaphorical according to this understanding? If by *model* we accept something like the Oxford English Dictionary definition, namely a "representation in three dimensions of some projected or existing structure, or of some material object, showing the proportions and arrangements of its parts" or "something that accurately resembles something else" then the answer is presumably in the affirmative. By referring to a set of mathematical equations as a model the idea is presumably to indicate that the former (set of equations) is thought to be like the latter (model) in also being a sub case or token of that class of phenomena that serve to represent, express or resemble existing or projected features of the real world.

But despite the seeming presumptions of some advocates of a rhetorical turn in economics, recognition of the metaphorical intent of talk of mathematical modelling does not in itself actually serve to justify or otherwise support the mathematical modelling emphasis of modern economics. This is so just because metaphorical reference can me wrong or inappropriate. It may be a (possibly mischievous) misrepresentation of John to call him a sheep; he may not after all be willing to follow others blindly. Far from reaching a ceiling, prices may be found to go upwards further still. The often proclaimed *death* of Keynesian economics appears currently premature (unless we prefer to think in terms of resurrection). And for the ontological reasons I have set out, it may be the case that mathematical deductive reasoning

[17] Of course, metaphor like most other categories is a contested concept. Those who reject a realist orientation will no doubt disagree with the interpretation accepted here. But then they will probably take issue with the whole discussion.

cannot usefully serve to represent or express social reality. Indeed, it seems to follow from such considerations that most of the time we ought not to refer to the economic formalism as models or modelling at all. No doubt, given the weight of established custom, the terminology of modelling is by now immovable. But if so (and for convenience I persevere here in calling these mathematical devices models) it seems we should at least accept that they inevitably mostly serve as poor models. In any case, whatever the appropriate language it is clear there is little salvation for the mathematical deductive *emphasis* of modern economics just through explicitly noting that the category of model in economics is metaphorical.

An entry barrier

Does the emphasis on mathematical deductive reasoning serve as an entry barrier? I believe it does, but it is important to be clear in what sense. An affirmative answer could be interpreted as accepting either (or both) of (at least) the following two interpretations: that the emphasis on formalism serves to exclude

1) individuals who are (or who are considered to be) insufficiently capable at mathematics
2) individuals (and groups) that wish to pursue alternative non-mathematical explanatory approaches in the belief that such alternatives are likely to prove at least as explanatorily fruitful

I think it suits many mathematical economists to suppose 1) is the dominant case whereas I think 2) is the more generic and the real problem. Certainly it is a problem. However, accepting this as a possibility directs us to a questioning of the use of power in the academy. I do hope it is not out of place to address this relevant issue in the current forum.

So far I have concentrated mostly on what might be termed the intellectual problem of modern economics, namely the misconception that methods of mathematical ‘modelling’ are a grounded, the best, and/or the only proper way of proceeding. But an additional, institutional, problem that explains why the failings of the mathematical project have not led to a flourishing of alternative approaches, despite the demonstrated explanatory fruitfulness of some of the latter, is precisely an institutional barrier to entry. The problem here is simply that those with institutional power allow almost no leeway for the undertaking of alternative approaches to formalistic modelling, despite the repeated failings of the latter, and indeed the demonstrated successes of alternatives (see e.g., Lawson 2009a or various contributions to Fullbrook, 2009). Those with power often act as very restrictive gate keepers.

This is a very significant obstacle to intellectual advance. I should perhaps stress that in observing this, I (of course) have no desire to replicate the problem in a different form. My own view is that all individuals should have a very real freedom to proceed as they see best fit. Certainly, I have no desire to see experimentation with formalism formally excluded. This is very far from my position.

That said, I do however suspect that if the noted dogmatism were overcome, if this gate keeping were to end, the emphasis on formalism would likely change very quickly without any 'legislation'. It seems to me anyway that many economists use mathematical deductive methods just because this is what is required of them, not because of any deep conviction in their relevance or utility. As noted at the outset, it is mostly only modellers that get appointments in university economic faculties; it is mostly only such modellers that get promoted; it is mostly only modellers that get research grants from certain sources; it is mostly only PhD and post doctorate research taking the form of mathematical deductive modelling that gets funded; it is mostly only this sort of research that can get published in core journals, etc. (This is presumably the reason too that many economic methodologists mostly hold back from criticising the mathematical emphasis). It is in this sense that the mathematical emphasis constitutes an entry barrier. Take away the insistence that only mathematical deductive methods be supported and rewarded in the economics academy and I strongly suspect the composition of academic identities and practices will change very quickly, even if most of the current individual practitioners stay in place. But here I merely speculate; primarily I want to emphasise that I see no reason for dogma and legislation of any kind.

Of course, all of us should strive to maintain standards and seek to justify what we do. But this is precisely what the mathematical modelling project currently mostly fails to do. Modern economists very rarely seek to justify the mathematical orientation of their endeavour, no matter what the extent of the failures of the latter. Nor is the orientation very often even questioned[18]. When the results achieved are not successful, the response is almost always either to find a different set of questions to tackle, or to develop a different set of formalistic 'models', or 'modelling' techniques, and so forth.

I might add that if some individuals sincerely believe that there is good reason why experimenting with formalistic models is best not only for them, but for the rest of us too, that we all ought to be doing only mathematics, I am again equally in favour of their receiving a platform; I support their being heard and accommodated generally. They may even be right, though I currently strongly doubt it for the reasons set out. The problems faced by the discipline stem not from a surfeit of arguments explicitly defending competing methodological positions but the current reluctance of proponents of the mathematical modelling emphasis to engage in methodological debate *whilst simultaneously withholding*

[18] Frank Hahn seemed to capture the prevailing sentiment when on the occasion of his retirement from Cambridge, he saw fit to contribute a piece to the *Royal Economics Society Newsletter* with the following explicit advice for modern economists: "avoid discussions of 'mathematics in economics' like the plague and give no thought at all to methodology" (Hahn, 1992). Elsewhere Hahn writes of any suggestion that the modern heavy emphasis on mathematics may be a problem that this is: "a view surely not worth discussing" (Hahn, 1985, p. 18). Hahn himself, I should add, failed to practice what he preached; he was actually quite open to methodological discussion. But his words are printed and widely supported.

opportunities and resources from those with different methodological convictions to themselves [19].

I might finally stress that in arguing for a more intellectual forum in the economics academy, in suggesting that we replace the specific methodological orientation that prevails with a more modest pluralistic orientation, I am arguing not against rigour, but against the naive supposition that it takes only one form. The position I defend does not even constitute an argument against the study of social phenomena being scientific in the sense of natural science. To the contrary, it grounds an argument that such study can be so scientific in the relevant sense, once alternative practices are facilitated; although that is another story (but see e.g. Lawson, 1997, 2003a, 2010).

Conclusion

As a final couple of comments I note that in recent months there has been increasing talk in economics of a need to turn both to Keynes, and, in some quarters at least, even to philosophy. For economists the philosopher who first comes to mind seems to be Popper. However, it tends to be Popper's early epistemological writing, particularly on testing and falsification that are referenced. Most economists overlook Popper's critical rationalism, and the fact that, throughout his life and increasingly so in his later years, Popper wrote on ontology. This is not a place to launch an analysis of Popper but it might be worth noting that he too came to emphasise the importance of the openness of the social world and so the futility of attempting to predict it. In brief in 1990 he writes:

> "Only the system of our planets is so well isolated from all the extraneous mechanical interference that it is a unique, natural laboratory experiment. Here, only the *internal* disturbances interfere with the precision of Kepler's laws... In most laboratory experiments we have to exclude many disturbing extraneous influences such as change of temperature or the normal moisture of air. Or we may have to create an artificial environment of extreme temperatures -- say, near to absolute zero....
>
> But what does all this show us. It shows us that in the non-laboratory world, with the exception of our planetary system, no strictly deterministic laws can be found" (Popper 1990, p 24)

And Popper is clear that it is not just an ignorance of future, but the openness of the future that is at issue:

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[19] It is not inconceivable, of course, that the noted intellectual failings and the institutional problems of modern economics are connected. The latter no doubt are a response to the former (as well as a cause of its continuance). If modern mathematical economics were more widely successful in providing insight then I suspect its proponents would be more susceptible to interaction, debate, openness and tolerance of others (which *is* of course the scenario we tend to find in the more explanatorily successful disciplines).

> “Quite apart from the fact that we do not *know* the future, the future is *objectively not fixed*. The future is *open: Objectively open*. Only the past is fixed; it has been actualised and so it is gone. (Popper, 1990, p. 18).

Or as Popper writes in the *Introduction* to his collection of essays systematised as *The Myth of the Framework*:

> "The future is open. It is not predetermined and thus cannot be predicted -- except by accident. The possibilities that lie in the future are infinite" (1994, p. xiii)

Hopefully enough has been said to suggest that a nod towards Pooper does not easily support the current emphasis on formalistic modelling and prediction (for a longer account see Lawson, 2008).

What about Keynes? As I write there is in the economics academy and elsewhere a clear renewal of interest in the writings of Keynes. As a final observation it is then perhaps of interest in this context to note that Keynes too held similar worries to those expressed above concerning the relevance of formalism to the analysis of social phenomena. I have noted that in order to guarantee successes with methods of mathematical deductive modelling, certain conditions are required that seem only rarely to come about. This was also Keynes' view[20]. In consequence, this may be an appropriate moment to recall Keynes' evaluation of the relevance of econometric techniques in particular, resting as these techniques mostly do, on the method of multiple correlation. The context in which Keynes makes his evaluation is in response to an invitation from the League of Nations in the 1930s to review Tinbergen's early econometric work on Business Cycles. Here 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 analyzed 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).

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[20] An assessment that such conditions are unlikely to emerge in the relevant contexts, underpins his critique of aspects of G E Moore's ethics (see Lawson 1993), his analysis of the relevance of probability judgements in his *A Treatise on Probability* (see Lawson, 2003b), and his critique of econometrics (Lawson, 2003a, 2003b). The latter was formulated even after the publication of his *A General Theory*, the book that so many cite as the inspiration for their 'Keynesian' modelling activities.

In my own analysis above I have identified, as the relevant conditions for correlation analysis to be guaranteed success, a world of isolated atoms. Perhaps viewing the assumption of isolation as obviously irrelevant, Keynes instead points to the need for a 'comprehensive' list of the required 'atomic factors'. But the underlying assessment is essentially the same (if a subset of all the potentially influential factors cannot be isolated from the others -- the objective of a well-controlled experiment -- then all must be included in any analysis). The point is that in examining the relevance of the method in question Keynes is concerned that it be appropriate to the material being studied, and he concludes that this is typically unlikely.

If the current crisis results in a shift in the economics academy in the direction of thinking associated with Keynes, the hope must be, then, that such a shift will be not to a form of mathematical modelling identified as Keynesian, but to a form of analysis that takes its leave from Keynes' critique of such modelling, certainly from a critique of any *insistence* that modelling of a mathematical deductive type is the only way to proceed.

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