

# The current economic crisis: its nature and the course of academic economics

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The current crisis has triggered significant debate concerning economic theory and policy. Largely absent from this debate is an informed discussion of the methods used by economists in analysing the economy and formulating their proposals. But method matters. Here I argue that current academic research practices need to be transformed before real insight can be achieved. Specifically, I indicate why and how a more grounded framework than that presupposed by current research practices facilitates a potentially more fruitful approach to understanding the crisis.

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## 1. Introduction

As the current economic crisis continues and seemingly deepens it is not unusual to find groups of academic economists being attributed a share of the blame. A significant amount of this emanates from other academic economists. Those whose proposals have been largely ignored by policy makers in recent times are now finding that their voices are being heard, at least in the popular media. And they are not refusing the opportunity to say ‘I told you so’, suggesting that if only those in power had listened to us or me, rather than to them or s/he, things would not have gone so wrong.

Mostly this criticism focuses on the substantive economic theories and policies that previously have been in favour. Seemingly little attention is given to the modes of analysis that have been utilised in support of these positions. Yet method matters. And in my assessment the sorts of methods that prevail in modern economics, whilst fundamental to understanding how recently prominent theories have been sustained, do not carry the warrant that their widespread usage seems to presuppose. In consequence, I am especially concerned that the critics avoid now filling academic journals with contributions that make the same more fundamental, essentially methodological, mistakes as their economic opponents, albeit in slightly different guise.

For many years now, economic policy analysis emanating from the academy has been framed mostly in terms of properties of mathematical deductivist models. This modelling activity has not provided too much insight (see Lawson, 1997, ch. 19; 2003A, ch. 1). The

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anticipated response against which I want to caution is the substitution of yet more formalistic models, albeit models reflecting alternative economic hypotheses, in place of those that have hitherto been dominant. For the more fundamental problem of recent years, I shall argue, is not so much the use of specific inappropriate models, but the emphasis on mathematical deductivist modelling *per se*. Such models can provide limited insight at best into the workings of the economy (or any other part of social reality). Indeed, I will suggest that the formalistic modelling endeavour mostly gets in the way of understanding.

Clearly an opening up of the economics academy to methods other than those of mathematical deductivist modelling, though an appropriate response if I am correct, will not, in and of itself, lead us out of the crisis. But in that almost all academic resources in economics have, in recent years, been devoted to this mathematical activity, the resources that would thereby be rendered available to relevant academic enquiry, a situation not occurring for very many decades, must be a stimulus to any solution.

I do go further and indicate an alternative non-formalistic framework that I believe can serve the purpose of analysing the current situation. I also identify various fundamental mechanisms and developments that have contributed significantly to the crisis. But this section is primarily illustrative. A comprehensive analysis would require a different paper.

## 2. Background

Before indicating why the emphasis on mathematical modelling in modern economics is problematic, let me first observe that in economic journals the formalistic modelling activities seem currently to be continuing unabated. If my methodological concerns outlined below are valid, and if the crisis provides an opportunity for change, the most prominent academic economic journals are seemingly not yet responding. However, academic articles tend to have a significant publication lag, and so it may be too early to say; to this point, in fact, analyses of the crisis appear to be mostly restricted to the popular media.<sup>1</sup> However there are academic economic papers in circulation on the internet that are seemingly intended for journal publication. Here, too, the modelling emphasis appears to be maintained. One paper though, and a seemingly influential one, does at least address the issue of economic modelling explicitly. And I think it may be useful to motivate my discussion by first very briefly focusing on this contribution, if only to indicate that my concerns cannot yet be put aside as no longer relevant.

The paper in question is by David Colander, Hans Föllmer, Armin Hass, Michael Goldberg, Katerina Juselius, Alan Kirman, Thomus Lux and Brigitte Sloth (2008), and is aptly titled *The Financial Crisis and the Systemic Failure of Academic Economics*. This paper, by a set of established, mainstream leaning, and clearly concerned, economists, seems by all accounts to have been especially widely circulated, and, as I say, is proving influential.<sup>2</sup> As such it may well be highly indicative of current thinking and evaluation.

<sup>1</sup> In consequence, the debate noted at the outset has not itself involved forms of formalistic modelling. Economists (academic or otherwise) have always been prepared to avoid the formalism when invited to comment in the popular press. Very often, though, those academic economists so invited have risen to prominence, and qualify for such invitations, just because of their contributions to mathematical-deductivist modelling. And typically they revert to their formalistic practices when returning their more 'serious' academic journal contributions.

<sup>2</sup> Although to my knowledge not yet published, the paper has circulated widely and proven very influential in heterodox circles especially, being announced by its authors as the 'outcome of a week of intense discussions'. In fact at the time of writing there are internet sites devoted to its discussion (see <http://economistsview.typepad.com/economistsview/2009/02/the-financial-crisis-and-the-systemic-failure-of-academic-economics.html>), whilst its 'access statistics' on other sites are significant (see <http://econpapers.repec.org/paper/kudkuiedp/0903.htm>)

As might be expected, the paper in question carries significant insight. Colander *et al.* inform us that they ‘trace the deeper roots’ of the crisis to economist’s ‘insistence on constructing models that, by design, disregard the key elements driving outcomes in real-world markets’ (p. 1). In their view ‘the current academic agenda has largely crowded out research on the inherent causes of financial crises’ (p. 2). And their central emphasis is that ‘[m]ost models, by design, offer no immediate handle on how to think about or deal with this recurring phenomenon’, namely crises (p. 2).

Getting down to details, these influential critics focus in part on the use of mathematical models by investors.<sup>1</sup> Mainly, though, Colander *et al.* focus on the formalistic modelling activities within the academy. Turning to specific claims built into modern economic models these authors single out the ‘the twin assumptions of “rational expectations” and a representative agent’ as particularly unrealistic. The assumption of rational expectations imparts a form of consistency between (i) the modeller’s conception of a section of reality, and (ii) the conception of reality held by agents whose expectations form part of the content of that model. It assures that, within any such model, expectations cannot be systematically wrong. The assumption of a representative agent allows that theorising need only be in terms of a single average or representative individual.

In challenging these assumptions Colander *et al.* call for more realistic specifications, based, as they present it, on empirical insight. They summarise reporting that ‘it seems to us that much of contemporary empirical work in macroeconomics and finance is driven by the pre-analytic belief in the validity of a certain model’.

Having identified some obvious problems with currently dominant economic practice, how do these authors suggest that matters be improved? Early on in the paper these authors make critical reference to their own conception of ‘standard models’. And they make it very clear that the only acceptable way of proceeding is developing new formalistic models in their place, ones that are appropriate for our current exceptional times:

The implicit view behind standard models is that markets and economies are inherently stable and that they only temporarily get off track. The majority of economists thus failed to warn policy makers about the threatening system crisis and ignored the work of those who did. Ironically, as the crisis has unfolded, economists have had no choice but to abandon their standard models and to produce hand-waving common-sense remedies. Common-sense advice, although useful, is a poor substitute for an underlying model that can provide much-needed guidance for developing policy and regulation. It is not enough to put the existing model to one side, observing that one needs, ‘exceptional measures for exceptional times’. What we need are models capable of envisaging such ‘exceptional times’. (Colander *et al.*, 2008, p. 2)

In the course of their argument, these authors go on to suggest a possible need for ‘a different type of mathematics than that which is generally used now by many prominent economic models’ (p. 3); and for giving back ‘an independent role to expectations in economic models’ (p. 9). In their view ‘[o]nce one acknowledges the importance of empirically based behavioral micro foundations and the heterogeneity of actors, a rich

<sup>1</sup> They suggest that the use of formalistic models to quantify and hedge risk has encouraged commercial banks, investment banks and hedge funds to use more leverage (i.e., borrowing money to supplement existing funds for investment in such a way that the potential positive or negative outcome is magnified and/or enhanced) ‘as if the very use of the mathematical methods diminished the underlying risk’. Quite reasonably, seemingly echoing recent formulations of Vinca Bigo (2008), these authors warn of the danger of what they call ‘control illusion’, suggesting that dominant emphasis on apparent ‘mathematical rigor and numerical precision of risk management and asset pricing tools’ results in a ‘tendency to conceal the weaknesses of models and assumptions to those who have not developed them and do not know the potential weakness of the assumptions’ (p. 6).

spectrum of new models becomes available [...] [allowing] one to study out-of-equilibrium dynamics and adaptive adjustments. Such dynamics could reveal the possibility of multiplicity and evolution of equilibria' (p. 9). They also conjecture, that '[i]f one accepts that the dispersed economic activity of many economic agents could be described by statistical laws, one might even take stock of methods from statistical physics to model dynamic economic systems' (p. 10). In terms of method, they 'recommend a more data-driven methodology' in which '[c]ointegrated VAR models could provide an avenue towards identification of robust structures within a set of data' (p. 11), adding that

A chain of specification tests and estimated statistical models for simultaneous systems would provide a benchmark for the subsequent development of tests of models based on economic behavior: significant and robust relations within a simultaneous system would provide empirical regularities that one would attempt to explain, while the quality of fit of the statistical benchmark would offer a confidence band for more ambitious models (Colander *et al.*, 2008, p. 11)

In short, these authors do not question the contemporary significant attention to formalistic modelling *per se*, but rather recommend the development of additional alternative mathematical models and techniques of mathematical-deductivist modelling.

### 3. What is wrong with economists' modelling?

So it seems that any concerns I may have about the emphasis on formalistic modelling, assuming they are valid, ought not yet be put aside as redundant. However, are these concerns valid? Are they justified intellectually? What is wrong with the sort of response advocated explicitly by Colander *et al.*—and, of course, implicitly by many others as a presupposition of their practices?<sup>1</sup> Why do I suppose that mathematical deductivist modelling of the sort pursued by economists is a problem in itself?

The basic answer, elaborated at length elsewhere in philosophical terms (see e.g. Fullbrook, 2009; Lawson, 1997, 2003A), can actually be put very simply. But before giving the answer, let me anticipate and try and pre-empt a possible misunderstanding. The fundamental problem of modern economics, as I see it, is the mainstream *insistence* that mathematical modelling is the only useful, and the proper, way to do economics. It is this *insistence* on mathematical deductivist modelling that I am primarily seeking to criticise

<sup>1</sup> This is true of some heterodox economists too. See, for example, the blurb for the clearly formalistic book by Carl Chiarella *et al.* (2009) (see <http://www.routledgeeconomics.com/books/Financial-Markets-and-the-Macroeconomy-isbn9780415771009>). Even Steve Keen's otherwise excellent recent analysis of the failure of modern economics to address the relevant issues seems to end up supporting a (different) sort of formalistic modelling: 'Fortunately, behavioural economics provides the beginnings of an alternative vision as to how individuals operate in a market environment, while multi-agent modelling and network theory give us foundations for understanding group dynamics in a complex society. They explicitly emphasise what neoclassical economics has evaded: that aggregation of heterogeneous individuals results in emergent properties of the group which cannot be reduced to the behaviour of any "representative individual" amongst them. These approaches should replace neoclassical microeconomics completely. The changes to economic theory beyond the micro level involve a complete recanting of the neoclassical vision. The vital first step here is to abandon the obsession with equilibrium. The fallacy that dynamic processes must be modelled as if the system is in continuous equilibrium through time is probably the most important reason for the intellectual failure of neoclassical economics. Mathematics, sciences and engineering long ago developed tools to model out of equilibrium processes, and this dynamic approach to thinking about the economy should become second nature to economists' (Keen, 2009, p. 5). At the same time, of course, there are heterodox contributions that avoid the formalistic emphasis. On the current crisis, see, e.g., Victoria Chick (2008), Sheila Dow (2008), and other contributors to the 2008, volume 27 edition of *Contributions to Political Economy*.

here. Although, for reasons I give below, I expect the enterprise to generate insight only rarely at best, I do not want to be equally dogmatic and assert that such modelling could never provide insight, or suggest that no one should ever experiment (see my various commentaries on this in Fullbrook, 2009, especially Lawson, 2009B).

My starting point, though, is that formalistic modelling has been mostly unsuccessful at providing insight (see Lawson, 2003A, ch. 1). The paper by Colander *et al.* discussed above points to the explanatory failures of the modelling project in times of crises, as well as to certain unhelpful unrealistic assumptions. The truth, though, is that the project of providing mathematical models of economic phenomena has proven to be explanatorily inadequate throughout its history, it is not something that has emerged with the crisis. Similarly, claims widely recognised as unrealistic are a feature of seemingly all mathematical deductivist endeavour in modern economics (again see Lawson, 2003A, ch. 1).

Why should this be? And specifically, why do I suppose that the emphasis on formalistic modelling is the problem? My answer, simply put, can be expressed in the following three propositions:

- (i) The sorts of mathematical deductivist methods that economists use are, like all research methods, types of tools.
- (ii) All tools are appropriate to dealing with but a limited set of tasks, involving a limited set of phenomena, in a limited set of contexts, and not others.
- (iii) The nature and conditions of social reality are such that the forms of mathematical deductivist reasoning favoured by modern economists are almost entirely inadequate as tools of insightful social analysis.

I doubt that many would suggest that we seek to use pencils to cut hedges, telephones to dig gardens, forks to fly us to other countries. Yet pencils, telephones and forks can be very useful to us in certain contexts, with respect to very specific tasks and phenomena. Marx long ago observed that ‘in the analysis of economic forms neither microscopes nor chemical reagents are of assistance’ (1974, p. 90). There is reason to believe that mathematical deductive methods are equally of little assistance in the analysis of most social phenomena.

Why so? First note that the sorts of mathematical deductive methods that economists use presuppose event regularities or correlations. They require that events or states of affairs are connected as empirical regularities. More specifically, they require regularities that connect events standing in causal sequence, in order to deduce that this event happened because of, or followed from, that event (for example the increase/decrease in consumption or investment or earnings came about because of the increase/decrease in income or interest rates or productivity).

Second, it follows that economists in their theorising must produce conceptions that are consistent with such event regularity presuppositions. They require theories that guarantee that for any given conditioning event (or set of outcomes)  $X$ ,<sup>1</sup> some predictable event  $Y$  inevitably follows (or at least does so on average, with something like a small distribution of outcomes around the mean).

Third, in practice economists meet this need by constructing theories couched in terms of isolated atoms. By atoms I do not mean something small, but something constructed as if it has the same independent invariable effect whatever the context. The assumption that

<sup>1</sup>  $X$  can of course represent a vector of events  $x_1 \dots x_n$ .

such atoms are isolated, means that, if triggered, their effects are unimpeded by other factors, and so are deducible, or predictable, and so mathematically tractable. It is like assuming economic agents are like wind-up toy dolls that, once wound up and placed on a reasonably smooth surface such as a table, predictably walk forward, if nothing intervenes in their path. In modern economics, the wind-up doll is typically (though not necessarily) the supposedly optimising (perhaps representative) agent in the context of a closed scenario with a unique optimum.<sup>1</sup>

Parenthetically, we can note that this sort of practice is illuminated by considering the experimental practices within the natural sciences. In the experimental laboratory, scientists do often succeed in isolating stable causal mechanisms, allowing the latter to be triggered and their unimpeded effects examined. The assumed-to-be isolated and atomistic entities of modern economics are treated as if analogous to the experimentally isolated, intrinsically constant mechanisms that are the focus of the (experimental) natural sciences.

#### 4. Critique

The essence of my criticism of the modelling emphasis is simply that the twin presuppositions of economic modellers that (i) empirical regularities of the sort required are ubiquitous, and (ii) social reality is constituted by sets of isolated atoms, are simply erroneous.

The repeated predictive failure of econometric forecasting models over the last 50 years is itself sufficient to cast significant doubt on the validity of the first presupposition. It is the empirical record that reveals that event regularities of the required sort are a relatively rare occurrence.

But actually, we can see that even in the natural sciences (where predictive testing is thought to be more successful), event regularities are not just sometimes observed in, but mostly *restricted* to, well-controlled experimental situations. This is precisely because conditions of experimental *intervention* tend to be required for intrinsically stable mechanisms to be isolated from countervailing factors. In the experimental laboratory it is indeed sometimes the case that event regularities are laboriously *produced*, correlating precisely the triggering of isolated intrinsically constant mechanisms and their unimpeded effects. But even in the controlled experiments of the natural sciences, attempts to bring about or reproduce such outcomes are also very often unsuccessful. Economists are thus seen to be rather heroic in assuming that scenarios that are laboured for in natural scientific experimentation, often involving great difficulty and subtlety, occur quite spontaneously everywhere in the social realm.

If event regularities hardly occur in the social realm, it does not take too much reflection to see that the second presupposition is also invalid, that the constituents of social reality can rarely be aptly portrayed as systems of isolated atoms.

<sup>1</sup> It may be thought (indeed a referee of an earlier version of this paper suggested) that general equilibrium theorising avoided the condition of isolation that I am identifying. But not really (see Lawson, 2005, 2007). The equilibrium framework typically comprises a number of equations. The theory underpinning each equation presupposes a closed world/system of isolated atoms. Equilibrium is typically a consistency criterion across (or a solution to) the set of equations. Moreover the question posed in such analyses is whether such a consistency property or 'equilibrium' or 'solution' exists as a mathematical feature of the equation system; there is typically nothing in the analysis to suggest that this is an outcome brought about by these individual atoms in the context of a world expressed (per impossible) by the set of equations.

If the ongoing crisis has served to emphasise one feature of the world economy it is surely that its numerous aspects or components are anything but isolated from each other. Developments in specific parts of the system have been found to immediately impact on developments in other parts, developments in one region to impact on those in others, and ultimately today's developments are closely related to what went before and will make a significant difference to the possibilities open to the future.

We cannot experimentally (or otherwise) isolate markets from monetary systems from firms and other processes of production, from state institutions including legislative and other regulatory processes, etc. Each is constitutively dependent on the others. And each human being is no less isolated. We are all of us inescapably socially situated and formed. We grow up to find ourselves gendered this way or that, of this nationality or that, and we become occupied as teachers or students, or as taxi-drivers or cleaners, as landlords/ladies or tenants, as employers or employees, etc. Each such position or status carries with it a set of community specific rights and obligations that bind us to, and indeed constitute us in relation to, others (teachers *qua* teachers only exist in relation to students, and vice versa, and similarly constitutive relations hold between employers and employees, landlords/ladies and tenants, etc.).

At the base of it all are accepted ways of doing things (some of which we call rules). We (as a community, either collectively or through representatives) accept in the sense of observe/act upon (whether or not we positively support or endorse) the creation of positions or statuses, that get allocated to certain individuals, and which are associated with positional powers or rights (allowed practices) and obligations (required practices). The network of accepted social positions and associated rights and obligations coordinates social life.

If social reality is composed of phenomena that are anything but isolated, so too everything is far from constant, or atomistic, but rather is in transformation. Think of language. The English language is a (typically unacknowledged) condition of the practices of all those currently speaking and/or writing English, and its reproduction and transformation is an (typically unintended) outcome of the same practices. This is how language exists: in process. That is its mode of being. It becomes and begets through time, a complex structure continually being reproduced and transformed through practice. But what is true of language is true of everything else that is social (that is, everything whose existence depends on us): seminars, universities, personal identities, cities, financial systems, climatic systems and, ultimately, capitalism itself.

Social reality, then, is a relational totality in motion. It is also has depth or structure; the social relations, rules, positions, power structures and so forth discussed above, are typically immeasurable, out of phase with the practices they condition (for example motorway driving and speed limits) and at best known, but not seen, to exist. And, of course, social reality involves meaning, and values and much else.

Social reality, in other words, is of a nature that is significantly at variance with the closed systems of isolated atoms that would guarantee the conditions of mathematical deductivist modelling. That is why modern economics has continually failed on its own terms. It is also why, as a step on the road to this failure, economics is *inescapably* profuse with assumptions accepted by everyone as widely unrealistic, including, but certainly not reducing to, those highlighted by Colander *et al.*

So the more pressing and pervasive problem of modern economics, I suggest, is not a case of *this* particular set of mathematical deductivist models requiring substitution by *that* set. It is the emphasis on mathematical modelling *per se*.

### 5. What of models that get the ‘right results’ or ‘address interesting questions’?

I should acknowledge that it is not only mainstream practitioners that will likely be resistant to (or uncomfortable with) my conclusions here. Heterodox economists *qua* heterodox economists, of course, differentiate themselves from the mainstream in *not* insisting that mathematical methods be everywhere employed (Lawson, 2003A, part III, 2006); pluralism in method is a characteristic feature of this heterodoxy (Lawson, 2009A). And much heterodox output is indeed non-formalistic. Nevertheless the ontological grounding of the rejection of mainstream (modelling) contributions is rarely explicit and not always fully recognised. In consequence, at least amongst some individuals within the modern heterodoxy, it is not always appreciated that the acknowledged widespread implausibility and failing of modern economics is not just a contingent feature of a dominant set of models but an almost inevitable consequence of the practice of mathematical deductivist modelling *per se*. In heterodox circles I frequently encounter remarks like: these particular models seem OK because they get the right conclusions, or they address interesting questions, or it is always necessary in social science to make thought-to-be false assumptions so mathematical modelling (although not a method to be insisted upon) is not worse than any other form of analysis, and so forth.

I am suggesting that all such ‘justifications’ are basically untenable, and I worry that their uncritical acceptance within parts of heterodoxy may in itself be a contributory factor to the current unhappy situation. With this in mind I think it cannot be emphasised too strongly that the practice of allowing assumptions known or believed to be false, whilst unnecessary (see Lawson, 2003A, ch. 4), allows more or less any conclusion to be deduced, without adding to the latter’s groundedness. In such a context, any evaluation that the question addressed is (or is not) an interesting one is simply irrelevant. Let me briefly elaborate.

Put starkly, if a desired and/or believed yet contested conclusion is derived by way of one or more claims that are believed to be false of our world, and of any really possible counterfactual world, but assumed in order to achieve model tractability, the only insight gained is into the properties of the model so constructed. Is there really much point to such an exercise? Certainly the analysis in itself provides no grounding or support for the preferred conclusion (however valid the conclusion may actually be). I suspect a supporter of a conclusion so reached would be dismissive if a second researcher used alternative but also accepted-as-false assumptions simply to undermine the preferred conclusion. But models that allow the deduction of a preferred and/or believed conclusion by way of claims accepted as wildly unrealistic are *per se* no more tenable or insightful.

Obvious though the above observations may seem, it is nevertheless by way of inserting known to be unrealistic assumptions that preferred positions are often ‘supported’. Consider the conclusion ‘capital markets result in the correct determination/pricing of risk and return’. This viewpoint is especially pertinent here just because the liberalisation or deregulation of financial markets that I come on to discuss below are supported in classical finance theory by precisely this claim that capital markets are ‘efficient’ in the sense of pricing correctly, where the models used to generate the conclusion are acknowledged to be false. It is clearly this reliance on formal modelling, necessitating unrealistic assumptions, that allows certain dominant viewpoints to be sustained. For example, we find William Sharpe, a Noble Memorial Prize Winner in economics, writing in his famous article on capital asset pricing that the inputs required to generate the conclusion are:

[. . .] highly restrictive and undoubtedly unrealistic assumptions. However, since the proper test of a theory is not the realism of its assumptions, but the acceptability of its implications, and since these assumptions imply equilibrium conditions which form a major part of classical financial doctrine, it is far from clear that the formulation should be rejected[. . .]. (Sharpe, 1964, p. 434)

I hope it is clear that any claimed support for conclusions preferred in heterodox circles, for example that 'Keynesian demand management policy results in non-inflationary economic growth' would be equally vacuous. Certainly, it is quite inconsistent to accept a model on the basis of its results and yet to decry opponent's models derived on a similar basis. In all cases, to accept a believed-to-be-false model just because doing so enables a sought-after result to be generated is little more than self-serving opportunism. Mostly this manoeuvre only misleads those overly impressed with the use of formalism.

Of course, the previous few paragraphs present matters in a rather stark manner and, it may seem, in an overly cynical fashion too, just because it is implied that in all such exercises any resulting 'conclusion' is chosen first and underpin the sorts of assumptions made (i.e., those that allow the desired conclusion to follow). The problem remains, though, that even where any conclusion is not determined *a priori*, but results *a posteriori* from a rather complexly presented analysis, so long as that analysis includes assumptions that are known to be false of this, and any really possible, world the analysis itself gives no added credibility to any conclusion drawn. Yet if the social world is an open, structured, totality in motion, then any insistence upon the practice of mathematical deductivist modelling, with its implicit presupposition of a closed world (or systems) of isolated atoms, means that, most of the time at least, unrealistic claims of the sort in question are unavoidable.

In short, once the constraint of seeking to be realistic is given up, and the prioritisation of formalistic modelling usually necessitates this, there are a multitude of ways of being unrealistic. A mathematical modelling (or any other) exercise, which unavoidably generates accounts of factors acknowledged as (wildly) unrealistic, provides no grounding thereby for the conclusions reached, whatever our independent evaluation of the latter. In consequence, to the extent that we are interested in the way the real world is and works, none of us should be any more interested in a believed-to-be unrealistic mathematical deductivist model that supports a preferred (e.g., believed to be true) conclusion, than one that seems to negate it.

## 6. 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 understanding the nature of the financial crisis. A major contribution in this regard, I think, is 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 something called 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 25 years or so, a period that 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; but this brief sketch does, I believe, indicate the relevance of the framework.

## 7. Contributory factors

Although it is not strictly necessary, let me, for illustrative purposes, give a very brief sketch of certain mechanisms, consistent with this framework, that likely have contributed to the current crisis.<sup>1</sup>

Situations where borrowing and investing are fuelled by expectations of rising prices, only to be met by (a set of events causing) a reversal of expectations and indeed price movements, a period in which offloading of financial assets (often very quickly) occurs,

<sup>1</sup> In doing so I am well aware that if I do not give any illustrations of the sort of mechanisms to which I refer, I am open to the (all too familiar) charge of being overly abstract and thereby somehow necessarily cut off from the real world. Equally though, if I do attempt an illustration, no matter how partial, I run the risk of encouraging the idea that that the analysis rests on the content of this illustration, so that any subsequent critique of the latter is interpreted as a critique of the overall perspective. Both lines of reasoning are fallacious of course (see the discussion in Fullbrook, 2009, especially pp. 76–7). But given that the economic crisis may be most readers’ dominant concern, let me throw caution to the wind and suggest a part of the substantive story of the crisis, hoping that the argument so far elaborated will not be thought to depend on the detail of what follows. For more detail, broadly consistent with the sketch provided here, see, e.g., Jamie Morgan (2009), which is especially comprehensive, or Peter Gowan (2009).

resulting in a movement typically interpreted as a ‘crash’, are referred to as economic or financial or asset-price bubbles.

In recent times, it is conceivable, and seemingly likely, that such bubbles in part at least have been intentionally brought about. Large and powerful North Atlantic, and in particular Wall Street, investment banks have repeatedly bought and sold financial and real assets to create and exploit price shifts. The stimulation of asset-price bubbles is a form of this ‘speculative arbitrage’.<sup>1</sup> Wall Street investment banks have been able to enter and influence specific markets, especially those emergent market economies of Eastern Europe with small bond or stock markets, first making large speculative profits, and then bursting the bubbles by withdrawing. With the later dot.com bubble these same banks found that they could equally gain financially from bubble bursting in home territory. The latest crisis must be seen, I think, as but the most recent bursting of a bubble, although this time with the banks themselves having been caught up in the fall out.

The bubble that has resulted in the 2007 credit crunch is significant not only for its size, but also for its nature. In previous over-lending induced crises both the source and scale of the problems have been easy to identify, allowing remedial steps of sorts to be taken. But at the heart of the 2007 debt bubble were the trading of *over-the-counter derivatives*, especially those taking the form of *collateralised debt obligations*, and these have served to confuse as to the true nature of the underlying situation

*Derivatives* are financial contracts or instruments that, as their name suggests, derive their value from that of something else. It is not necessary to own that ‘something else’. So trading in derivatives is a form of pure gambling. Indeed, a bet placed on a horse in a race, where the horse is not owned, is essentially a form of derivative.

The *over-the-counter* nature of the derivatives means that trading is directly between two parties, with no exchange trading allowing the determination of ‘market’ prices. The over-the-counter derivative market is the largest market for derivatives, and, with most of the traders made up of banks, hedge funds and such like, the market is largely unregulated with respect to disclosure of information between the parties.

*Collateralised debt obligations* are a structured form of asset-backed security. They are asset backed in the sense that their worth and payments derive from a portfolio of fixed income underlying assets. They are structured in the sense of being divided into prioritised tranches with each tranche being assigned a different degree of riskiness. Those tranches that are considered the safest, designated ‘senior tranches’, receive the lowest rate of return; those that are the riskiest, designated ‘junior tranches’, generate the highest premium payments.

As I say collateralised debt obligations seem to have been at the centre of the recent crisis. And the process by which they were assessed for their ‘riskiness’ is a significant factor in understanding how.

It is important to recognise that the tranches were prioritised not in accordance with the values of the securitised assets underpinning (all) the various tranches, but according to how the different tranches were to absorb any losses emanating from the underlying portfolio. Specifically, where only some underlying assets defaulted the losses were in the first instance born by the most junior tranches. The senior tranches only absorbed losses after the junior claims had been exhausted. As the number of defaults increased, increasingly more senior tranches suffered too.

<sup>1</sup> On this see also Nasser Saber (1999).

In short, the junior tranches generated higher premium payments, but the downside was that if defaults relating to the underlying assets occurred, the holders suffered losses *before* the holders of the senior tranches were similarly affected. Alternatively, the holders of the senior tranches, the supposedly least risky ones, received a lower risk payment, but would be the last to have their payments lost through non-performance of the underlying assets.

How was the riskiness of the tranches determined? This was undertaken by rating agencies, the main three being Moody's, Standard and Poor's and Fitch. Traditionally these agencies had focused their rating activities mostly on single-name corporate finance. However, with the rise of structured securities markets their activities expanded to include the evaluation of these assets. Somehow, these agencies expected to make meaningful independent evaluations of the creditworthiness of various entities, and rank them according to the rating scales AAA, AA, A, BBB, BB and so forth.

Very often these larger rating agencies offered 'credit rating advisory services' that essentially advised an issuer on how to structure its securities so as to achieve a given credit rating for a certain debt tranche. In consequence, it seems not unlikely that if the agency's advice was followed, the latter agency consequently felt obligated to provide the issuer with precisely that given rating.

Not surprisingly the banks choose very often to structure in such a manner as to achieve tranches with AAA ratings. In fact, in the last ten years the repackaging of assets has resulted in huge quantities of AAA-rated securities. Indeed, at one point roughly 60% of structured products were triple-A rated according to Fitch Ratings (2007) compared with less than 1% of corporate bond issues. And one result of all this was the generation of a perception (as it turned out, an illusion) that structured securities were comparable in terms of safety or riskiness with single name corporate finance.

Notice, once more, that a triple-A rating of a tranche did not mean that it was necessarily backed by prime loans or assets. Indeed, it was quite possible for the opposite to be the case. It meant only that if the underlying assets stopped performing, the holders of the lowest rated tranches would lose out first, whilst holders of an AAA-rated tranche would continue to receive income the longest. If all the underlying assets failed at the same time, due, say, to a significant economic downturn, then clearly all tranches would become worthless simultaneously.

Of course, the analysis of the nature of social reality discussed at length above reveals that all risk assessment that purports to attach numerical probabilities or other measures or grades to assets is questionable. It is not just in the academy that such activities are pursued where conditions are *prima facie* inappropriate. Nevertheless the rating agencies purported to possess the relevant expertise to assign meaningful credit ratings and indeed profited from seeming to do so.

Furthermore, many potential investors were convinced by their results. Certainly a significant expansion in the issuance of structured securities was stimulated by a seeming ability on the part of investment banks to repackage otherwise risky collateral in this way to create supposedly 'safe' assets. Many of the tranches were viewed by investors as almost free of risk, and, as noted, effectively certified as being such by rating agencies.

Clearly, this process of pooling and repackaging of cash-flow-producing financial assets into structured securities, with the intention of their then being sold on to investors, was highly lucrative to many parties involved while the situation lasted; numerous agencies had incentives to facilitate a large flow of loans through the system, whatever their worth. High fee payments (which could not be recovered if things later went wrong) were received by mortgage brokers for providing the initial loans, by investment bankers who repackaged

them as securities, by banks and other specialist groups who serviced them, by rating agencies who gave them high ratings, and then by insurance companies who guaranteed the holder of such securities against credit default.

But an additional advantage to the banks of all this lay in the the regulatory arbitrage that the collateralised debt obligations provided. Banks were able to expand leverage [the relationship of their assets (the amount lent out) to their equity or capital] in ways that were previously impossible. This expansion of leverage was a significant factor in allowing the investment banks and others to achieve greater pricing power in their trading activities, underpinning their bubble-inducing activities.<sup>1</sup>This is a complicated story that I can only briefly touch upon here.

Banks, of course, are, for various reasons, required to hold a minimum ratio of ‘capital’ to total assets, as a buffer against losses. Under the Basel accord,<sup>2</sup> banks are expected to maintain a minimum 8% buffer against a supposedly risk-adjusted measure of their assets (i.e. of their loans to others). Mostly, and particularly in the USA, this minimum ratio is interpreted as a target of 10%.

Clearly, any holding of capital is costly to the banks in the sense that it cannot be lent out at an interest. Yet over the recent past the creation of asset-backed securities of the form discussed above allowed the banks to increase their leverage substantially. It is important to see how this was achieved.

First, the very practice of securitisation made it possible for banks to avoid holding capital through their effectively becoming underwriters that, although making loans, could sell on these same loans to others. In this way assets became removed from the bank’s balance sheet, so that formally there was no need for a percentage of their value to be held as a capital reserve (it is because of securitisation that there are significantly fewer deposits in the modern financial system than there are loans).

It is significant for what ensued, however, to note that the banks nevertheless held onto many of the asset-backed securities.

In part this was achieved through many banks creating off-balance sheet ‘special purpose vehicles’ or ‘conduits’, including structured investment vehicles. In this process the banks still brought risk back onto themselves, even if not shown on the balance sheet, by ways that are rather complicated and need not be entered into here (see, e.g., Gowan, 2009).

Other banks, although also making loans and moving them off balance sheet by way of securitisation, chose not to create special purpose vehicles but instead to reinvest in triple-A rated tranches of those same or similar products. For the AAA ratings of these securities meant a substantially lower capital requirement (as well as a premium on an apparently riskless security). The Basel accord in fact required of commercial banks that, for AAA-rated securities, the amount of capital held in reserve need only be half of that required for ordinary commercial or mortgage loans.

In addition, in 2004, Hank Paulson persuaded the Securities and Exchange Commission to pass measures relaxing the restrictions on leverage for large investment banks. Thereafter, these firms were allowed to decide their own leverage supposedly on the basis

<sup>1</sup> In addition bankers, being paid on short-term cash bonuses rather than long-term profitability, had little incentive to make allowances for a future possible scenario in which investors no longer wanted to hold onto the asset-backed securities.

<sup>2</sup> A set of agreements set by the Basel Committee on Bank Supervision (so called because this Committee maintains its secretariat at the Bank of International Settlements in Basel, Switzerland and normally meets there) providing recommendations on banking regulations in regards to capital risk, market risk and operational risk. The purpose of the accords is to ensure that financial institutions have enough capital on account to meet obligations and absorb unexpected losses.

of their risk models (Gowan, 2009, p. 15). This facilitated an even larger leverage than for commercial banks.

## 7. The crisis

An outcome of all this was that collateralised debt obligations and other derivatives were distributed widely between the dominant institutions in the financial system, in particular the investment banks, as well as to their associated bodies including structured investment vehicles.

As it happens collateralised debt obligations were frequently made up of bundles of hundreds of loans of varied quality. They were perceived as relatively safe because, as noted, the rating agencies gave them a high rating. But in truth, the products so bundled came from hundreds of thousands of unidentifiable sources, and their credit worthiness and cash flow possibilities could not be determined. Being more or less completely opaque to those who bought them, and seemingly often intentionally so,<sup>1</sup> they were, at best, highly risky and in fact extremely precarious.

The situation was not one that could be permanently sustained. And in late 2007 the whole financial network came under strain, as various events, perhaps most notably a suspension of redemptions by BNP Paribas,<sup>2</sup> caused the viability of numerous financial instruments to be questioned. Soon the financial system as a whole was in some turmoil. Ratings of numerous financial assets were revised downwards; even many supposedly low risk super-senior debt tranches of the collateralised debt obligations came to be attributed junk status.

Once the money market and pension funds saw that some of the collateralised debt obligation arrangements were untenable, and then came to realise that there was no way of determining how much of the rest of the stockpile of collateralised debt obligations were more or less worthless, they offloaded them quickly. The withdrawal of these funds from Wall Street investment banks and associated entities made it impossible for the banks to sustain the collateralised debt obligation 'market', thus contributing to the crash.

<sup>1</sup> According to Satyajit Das (2006) 'in the [over the counter] market, dealers [ensure] that the clients do not know the true price of what is traded. The lack of transparency lies at the heart of derivative profitability. You deny the client access to up-to-date prices, use complicated structures that are hard for them to price, and sometimes just rely on their self-delusion' (p. 126). The same point is argued at length by Frank Partnow (2003).

<sup>2</sup> On August 9, 2007, the French BNP Paribas found it could not evaluate the market-to-market values of their securitised investments backed by subprime mortgages in three of their funds. In consequence subscription and redemptions were suspended. A press release by BNP Paribas included the following statement: 'The complete evaporation of liquidity in certain market segments of the US securitisation market has made it impossible to value certain assets fairly regardless of their quality or credit rating. The situation is such that it is no longer possible to value fairly the underlying US ABS assets in the three [...] funds. We are therefore unable to calculate a reliable net asset value ("NAV") for the funds. In order to protect the interests and ensure the equal treatment of our investors, during these exceptional times, BNP Paribas Investment Partners has decided to temporarily suspend the calculation of the net asset value as well as subscriptions/redemptions, in strict compliance with regulations, for the following funds:

- Parvest Dynamic ABS effective 7 August 2007, 3 pm (Luxembourg time);
- BNP Paribas ABS Euribor and BNP Paribas ABS Eonia effective 7 August 2007, 1 pm (Paris time)'. See [http://www.info-financiere.fr/upload/FCCNS005647\\_20071016.pdf](http://www.info-financiere.fr/upload/FCCNS005647_20071016.pdf). In fact BNP's decision followed a similar one a week earlier by German fund manager Union Investment, which suspended redemptions in one of its funds that had exposure to the US subprime market through ABS investments. Union Investment argued that it did not want to be forced to sell assets in a market that would command steep discounts. Within days of BNP Paribas's decision several other European firms followed suit and froze funds.

When the Wall Street banks tried to off-load their collateralised debt obligations, they found there were no takers for them. So the large banks too became victims of the crisis.

Of course, in theory the banks should not have been holding these assets. *Prima facie* the purpose of securitisation was to spread risk by distributing it away from the investment banks and across a large number of other investors. But the banks, as noted, kept the risky assets themselves. They used securitisation instead to circumvent the requirements on holding capital reserves, and thereby to expand their leverage in order to influence market pricing (whilst individual bankers, being remunerated through cash bonuses for short term gains rather than on the basis of long term profitability, had every incentive to enter into the risks involved).

The practices of credit rating agencies, too, have been a significant contributory factor to the whole situation. Had these agencies not been so willing to give ratings to illiquid, non-transparent, structured financial products such as collateralised debt obligations, and in particular had they not given such clearly unjustifiably high ones (for example had securities initially received the sorts of ratings they now carry), many pension and mutual funds would have been constrained by their own rules from buying them in the first place. These illiquid products are not easily traded on exchanges because there is no meaningful way to evaluate them. Thus, demand for them could not have developed in the way it did, had the rating agencies not provided the backing for them. Yet, as we have seen, the agencies were induced to give AAA ratings to a huge percent of tranches of collateralised debt obligations. And, to repeat, financial analysts, regulators and investors acted as if these ratings were as solid as the AAA ratings given to the safest corporate and government bonds. This allowed the financial boom in mortgage based securities to take place.

## 8. Background

All this is only a part of the story at best. And it begs many questions. How, for example, were the large investment banks able to generate the conditions described in the first place? An account of this would no doubt focus on the financial developments often systematised as economic or financial globalisation. These developments, notably the fiat dollar system, the ending of capital controls, and the free entry and exit of the major banks or operators in other financial systems, have undermined the capacity of most states to underwrite and control their own financial systems.

The volatility in foreign exchange markets following the breakdown of Bretton Woods, along with financial liberalisation, especially the abandonment of credit controls and the opening up of national financial systems to US operators, afforded an opportunity for a large and profitable expansion of Wall Street trading. It was these developments that, from the mid 1980s, allowed investment banks (traditionally companies that merely assisted other companies in raising financial capital in the capital markets, through such means as the issuance of stocks and bonds) increasingly to switch from trading securities on behalf of clients, to proprietary trading, that is, to actively trading various financial instruments with their own money as opposed to their customers' money, so as to make a profit in this manner for themselves.<sup>1</sup>

Through a series of 'financial innovations', some of which I have touched upon (involving the creation of new products and processes, institutional restructuring and

<sup>1</sup> The evolution of proprietary trading by investment banks eventually reached a point whereby the latter employed multiple desks of traders (often considered internal hedge funds within the investment bank, performing in isolation away from client-flow traders) devoted solely to this activity.

oversight structures), Wall Street investment banks were able largely to escape regulatory constraints and expand their activities and profits significantly. They even constructed a shadow banking system in London alongside the regulated sector, and eventually pushed aside the local agencies and came to dominate the square mile.

I will not go on. Hopefully the brief sketch is enough to convince that the conception of the nature of social reality outlined above is likely relevant to understanding the story. To repeat the cautionary remark noted earlier, the details of the recent period are complex, and a full understanding requires, amongst other things a detailed analysis of structural transformations of the financial sector, along with the various mechanisms whereby the credit/debt was created; however, a focus on these sorts of issues does seem essential to the story.

## 9. Implications

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.

Pace Colander *et al.* (2008), it seems overly heroic to suppose that in order to capture the sorts of developments that occurred, all that is required of modern academic economics is a different type of mathematics, or internal ‘theoretical’ adjustments like the treating of a model’s still isolated atoms as heterogeneous or as forming independent expectations; or focusing on the possibility of multiplicity and evolution of equilibria; or hoping that cointegrated vector autoregression (VAR) models will uncover robust structures within a set of data, and so forth.

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 atoms, but with the perpetual emergence of novelty, not least at the level of relational structures,<sup>1</sup> 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 and perhaps thereby predict crises and such like, but to understand the ever emerging relational structures and mechanisms that render them more or less feasible or likely.

<sup>1</sup> Indeed, in concentrating on mechanisms central to the crisis above I have perhaps underplayed the relationality of the picture. In the financial system, as elsewhere, everything is tied into everything else. Consider a situation wherein a pension fund  $P_1$  wishes to invest billions of pounds sterling, say, in company C, but is constrained by its rules from doing so because the company only has a rating of BB. However, it can invest in this company if at the same time it takes out insurance (a credit default swap) with an AAA-rated insurance company X against company C defaulting. For, if C does then default, X will, in theory, pay up, and X is considered reliable because it is triple-A rated. Now not only may X not actually have the capital to pay up if company C defaults, but even if it does, very many pension companies,  $P_1, P_2 \dots P_N$ , may have simultaneously insured with X to cover themselves if company C defaults. This set-up is a nice little earner for X as long as C flourishes, but underpinning the situation may well be a debt obligation that X could not meet, should company C default. Even more striking, in order to insure against C with X, it is not necessary to have a stake in C. A hedge fund, H say, may just decide that company C seems likely to fail, and take out an insurance (credit default swap) to cover this happening. That is, H may essentially bet against company C’s survival (just as X is essentially betting on the contrary). If C defaults there can clearly be a cascade effect. Even if X can afford to pay out to all companies that have taken out an insurance against this, its resulting loss of funds may cause the rating firms to downgrade it, causing pension funds to pull out and so forth. It will be clear that such a system is extremely highly interdependent, an unstable network in which a significant development in any one part will quickly reverberate throughout.

Amongst other things, this 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.

Given this sketch briefly set out, it seems likely that any response to the crisis by those in positions of power will include seeking ways to (i) recreate trust and confidence on the part of creditors and (ii) transform modes of regulation to better control excesses on the part of creditors. Whatever measures will be imposed, greater ‘transparency’ of financial practices will no doubt be sought. More fundamental questions too may be raised. Should the credit institutions of capitalism facilitate social and economic development or be mainly concerned, as at present, with advancing funds to those concerned merely with making more money? Should nation states seek to regain more control over the banking system, not least as a seemingly more reliable means of supporting the financial system as a whole, and is the latter any longer even feasible in western industrial economies?

As it happens, the sorts of implications and concerns rendered likely given the above basic analysis do seem to be on the minds of many of those in positions of governmental or policy responsibility. But gaining clarity is not helped by debates over whether one form of largely irrelevant mathematical modelling endeavour better expresses the measurable aspects of human economic activity than another form of the same endeavour.

Although my purpose has not been to pinpoint the issue here, it seems likely that a reliance on formalistic modelling is as misleading in the finance industry, not least in the practices of the rating agencies, as it is in the academy. However, a systematic exploration of this issue must await another occasion.

Focusing for now on the economics academy specifically, it remains the case that the latter is dominated by a mainstream project that rarely allows courses to be taught that are not mathematical modelling oriented, even to undergraduates; and performance everywhere is measured only in terms of formalistic modelling output. The latter is regarded as fundamental for research recognition, academic appointments, promotions and everything else. The appropriate response here is clear and does not need spelling out. Suffice it to say that an intellectual opening up of the economics academy would be revolutionary indeed, allowing at least the possibility of genuine debate on all issues and the promise of progress and a freeing up of resources for relevant research that have long been allocated for practices that have little if any grounding or rationale or obvious practical benefit.<sup>1</sup>

## 10. Final comments: a return to Keynesianism in the academy?

As I write there is, in the economics academy and elsewhere, a renewal of interest in the writings of Keynes. Economic historians and post-Keynesians are indicating and lamenting the decline of Keynesian teaching in universities over the last 30 years or so; policy-oriented economists are seeking to revert to policies described as Keynesian. The *Economist Magazine* even recently hosted an online debate on the topic ‘We are all Keynesians now’.<sup>2</sup>

<sup>1</sup> Of course, there are numerous further questions of relevance that cannot be addressed or even touched upon here, not least the question of how the economics academy arrived where it is, and how the situation is sustained. I have pursued these questions elsewhere (see especially Lawson, 2003A, ch. 10). For now it is enough for my purposes to emphasise the nature of the problem, and to caution against repeating the same essentially methodological errors.

<sup>2</sup> See <http://www.economist.com/debate/overview/140>

My concern is that in all of this the central problem may still be being overlooked. Given these developments, it is perhaps of interest to note that Keynes himself 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 deductivist modelling, certain conditions are required that seem rarely to come about. This was also Keynes' view. 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.

Given ongoing developments, this seems an opportune 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. (Keynes, 1973, pp. 285–6).

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 the potentially influential factors 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.

The hope has to be, then, that if the current crisis results in a shift in the economics academy in the direction of thinking associated with Keynes, it will mostly be a shift 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 deductivist type is the only way to proceed.

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