Does economics need to be reconstructed at the core?

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Is a scientific economics possible? – or desirable?

There has been a long history of discussion as to whether social “science” is possible. Many issues have been involved, including the extent to which events in the social realm are predictable, the difference between causation in the natural sciences and agency in human affairs, and the role (if any) of a sympathetic understanding of the motivations of social actors (*verstehen*). The aim of this paper is more modest, and does not cover all these topics, nor does it review the rich literature on the philosophy of economics. It merely asks what lessons one particular social science, economics, can learn from methods that the natural sciences, and especially biology, have used so fruitfully in understanding how the world works.

Within economics the focus is on theory – causal explanation and modelling – especially core mainstream theory, as depicted in textbooks, on the characteristics of markets and firms that affect growth and bubbles; macroeconomics and econometrics, for example, are not covered. Since this type of theory is widely regarded as the core of orthodox economics – macroeconomists tend to refer to the desirability of “micro foundations” – this focus goes to the heart of the discipline.

One motivation is that some economists claim economics to be scientific in some sense, so the issue needs to be considered to what extent and in what ways this may or may not be justifiable. It is true that important elements appear to resemble natural science, and indeed are borrowed from it, but I will argue that the resemblance is only superficial – at a more fundamental level the causal processes are quite different.

A second is that orthodox economics is widely perceived as not being fit-for-purpose, that purpose being to understand how the economy works – a perception shared not only by non-economists but also by many economists, as exemplified by the Institute for New Economic Thinking, which includes Nobel laureates (INET, 2010). A substantial minority of economists now identify themselves as “heterodox”, in opposition to mainstream economics; they tend to belong to organisations such as the Association for Heterodox Economics (AHE) and the International Confederation of Associations for Pluralism in Economics (ICAPE).

It could also be argued that the events in the economy in recent times, together with the almost-universal inability of the economics profession to understand what was happening until too late, reinforces that view. Variants of such a position have been put forward by such eminent mainstream economists as Krugman, Shiller and Eichengreen. Alan Greenspan (2008) stated that “he had put too much faith in the self-correcting power of free markets”, and that he had “found a flaw” in his ideology adding “I’ve been very distressed by that fact”. And a feature in *The Economist* magazine (2009) – a bastion of free-trade orthodoxy – recognised that modern economic theory “went wrong”; they located the problem in two branches, financial economics and macroeconomics, saying that they are rightly being re-examined. But the centrality of the bubble phenomenon to the financial crisis suggests the need to revise fundamental ideas about the nature of markets, a topic central to economics, something that mainstream economists have long resisted.

A third motivation, related to the second, is that it is important to go beyond reactive mode, in which standard theory perpetually and fruitlessly confronts direct criticisms of how it is done. An example is the issue of whether or not assumptions need to be realistic; being locked in this dispute is liable to distract participants from the deeper question, whether it would be better to base theory on evidence and place less reliance on assumptions of all kinds.

This paper therefore compares one particular natural science, biology, with the practices of economics, hoping to act as a guide to a better methodology. The focus on methodology rather than substance is important: biological processes are not a good model for social processes, as the causal mechanisms are quite different (Hodgson, 1993). The analogy is at a more abstract level.

In taking biology as the model, a judgment is clearly involved that it is successful, whereas economics is not, or rather, less so than it could be. The first part of this is easy to justify: biology has developed a large body of causal knowledge, covering for example how the body works (physiology, biochemistry, cell biology, etc), and evolutionary theory which explains how the diversity of living forms originated. The basis of this knowledge was accumulated in a remarkably short time. In 1855 biology could scarcely be said to exist. By 1955 the work of Mendel, Darwin, Snow, Pasteur, Koch, Bernard, Ross, Krebs, Crick, Watson and others had established the laws of genetics and evolution, and their inter-relation (the neo-Darwinian synthesis), plus the main features of physiology/biochemistry, including the mechanistic basis for the neo-Darwinian synthesis, as well as a substantial body of knowledge about the causes of disease. The success of biology has resulted from the ability of biologists to generate theory from evidence, even when confronting a reality of bewildering diversity and change.

A second judgment is that the two are sufficiently similar for the analogy to be useful. This has been the view of several important economists, most notably Marshall (1885), as well as non-economists (Gillies, 2004). One reason is that causal relations in biology are complicated and multiple as well as often stochastic, as in economics and more generally in the social sciences, albeit that biology has the advantage of a physical basis and a greater degree of regularity than the social world. Another is that both economic and biological systems are dynamic; and that they share the feature of open-endedness, i.e. that the future is not wholly determined by the present.

The arguments presented here do not require that social sciences in general, or economics in particular, need to be seen as being just like the natural sciences. Those who believe that it is undesirable for the social sciences to attempt to be like the natural sciences, or that the very idea of a social “science” is misplaced, might still agree that some useful lessons can be drawn from the comparison – if only to understand why the mainstream economists’ claim of similarity is false. My own view is that similar methods can be used in the social and natural sciences when they are appropriate to the subject matter. However, social science is more difficult, because it involves human agency as well as the type of causation invoked by natural sciences, and this introduces complications.

Before starting the main discussion, the issue of values needs to be raised. It is assumed here that the aim of an economic account is to illuminate the processes involved, not to pass judgment on them nor to use economic events to try and justify a particular moral and/or ideological position. The aim then is to produce an account which finds as much agreement as possible about how the system works, because it corresponds well with the real world. This can be personally difficult, as the task of disentangling what appears to be true from what one would like to be true is unpleasant, even painful, requiring moral courage. It is almost inevitable that the account that emerges will be distasteful for people of various moral positions; for example, a free-marketeer will dislike some aspects, a traditional socialist will dislike others – but they may still agree that, like it or not, this is how things are. Get over it.

Is economics not already scientific?

First, what do we mean by “economics”? Contrary to the impression gained by many non-economists, it is far from monolithic (Mäki, 2002; Davis, 2006). As an academic discipline, it may well be the most diverse of all, including as it does:

- neoclassical (traditional textbook) microeconomic theory and macroeconomics
- econometrics and other statistical methods
- game theory
- information economics
- specialised areas e.g. transport, healthcare, business
- behavioural economics and neuro-economics
- experimental economics
- institutional economics, evolutionary economics and historical accounts
- many other “heterodox” traditions, including post-Keynesian, feminist, Marxist and Austrian perspectives.

And yet it is no accident that many people believe economics to be monolithic. Economics textbooks, and the courses that use them, provide a particular view of the discipline which is widely regarded as its core. Many people have studied economics but are not professional economists, and this is the view that they receive of what the subject is about. When I refer to the possibility of making economics more scientific, it is “mainstream” economics of this type, as represented in textbooks and standard training courses, that I mean.

As is clear from the above list, research in economics is very different from the image that is portrayed by this mainstream view; and much of it is of excellent quality. In addition, many practising economists dissociate themselves from some or all of the textbook dogma, and/or its consequences in non-academic public debate. The central issue here is the view that “the market” is self-adjusting and produces optimal outcomes, at least under specified conditions such as “perfect competition”. This dissociation is itself intriguing – no biologist disavows the standard account of biology given in textbooks, except to say that it is outdated and/or over-simplified, and the same is true in all the sciences.

It is instructive to compare economics textbooks with those of the established natural sciences. On the surface, economics and physics textbooks are quite similar-looking, with a predominance of theory and mathematics, whereas those in chemistry and biology have a great deal more empirical content. Does this mean that the claim of the scientific nature of mainstream economics could be valid, if the processes described are sufficiently regular? A closer look shows that even this defence is invalid: the two types of textbook differ radically in the ***basis*** for the theoretical and mathematical statements. With physics they are empirically based, the result of centuries of observation and experimentation. Economics textbooks are typically almost free of empirical content. Where it occurs, it is included as case-study material for ***illustration***, and no attempt is made to ground the theory in reality. Unlike any of the sciences, but like mathematics, it is grounded in axioms that have been derived from idealised formalism rather than from evidence.

The role of description & experimentation

Natural scientists have one methodological tool that is of only limited use in the social sciences: experimentation, the ability to manipulate nature by altering some conditions while holding others constant. But this advantage is easy to overstate, because a great deal of the empirical content of the natural sciences results not from experiments but from careful and systematic description. Astronomical observations played a major role in the development of physics. Biology in particular is largely a descriptive science, from van Leeuwenhoek’s discovery of a previously unsuspected world of invisible organisms using the newly-developed microscope, through Darwin’s painstaking work on the subtle variations within and between species that gave rise to his evolutionary theory, up to today’s research, for example using microarrays to study gene expression.

The problem in economics is not so much what is impossible, but what is possible yet not practised. Economic history has a strong tradition of accurate description, and much of this goes beyond telling an individual story to the comparative study of different experiences, usually in terms of countries (Maddison, 1964; Maddison, 1969; Maddison, 1970; Landes, 1998; Pomeranz, 2000). Unfortunately, historical accounts have in general not explicitly informed core mainstream theory, nor been used to subject it to systematic revision.

Within economics too, there is a great deal of excellent empirical work, of an observational kind and also the increasingly influential experimental economics, in which a group of subjects is given a structured task (Smith, 2008). In addition “natural experiments” are widely used, in which causal inferences can be made from events in the real world that were not created by the scientists. A nice example is a study of the effect of family size on the mother’s work status: to distinguish a direct causal effect from confounding (e.g. her preference for career as against childbearing) and from reverse causation (e.g. promotion leading to a decision not to have a further child, or not yet), the authors used the sex of the first two children as a natural experiment (Angrist and Evans, 1998). If they were of the same sex, the parents are more likely to want another child, for reasons unconnected with the labour market, so this plays the same role as deliberate assignment would if it were possible.

The issue is that ***evidence is not systematically used as an input to core theory and modelling***, which is regarded as central and high status. It does not have a fundamental place, but tends to be restricted to relatively specialised areas, as in the example just given, or in studying the effects of education/training on later earning capacity. In particular, evidence is not allowed to question the central theoretical foundations that have been passed on for generations.

One topic that could usefully be presented descriptively, and which would inform theory building, is what types of market are, or are not, prone to bubbles. This phenomenon is increasingly well understood (Kindleberger, 1989; Shiller, 2005; Levine and Zajac, 2007), but if it is an inherent property of markets this would radically alter the self-regulating notion of markets depicted in textbook theory, and held as a default belief by many economists. On the other hand if it is restricted to certain types of market, the implications would be different but could still be far-reaching. One suggestion is that liquid trading markets are especially bubbles-prone (Turner, 2009); another possibility is that markets for which the price has no well-established relationship with costs are likely to develop bubbles. The issue is not whether economists are studying bubbles – which they now are – but whether their findings are being used to challenge traditional beliefs about the fundamental properties of markets.

Another is the competitive behaviour of firms and the resulting distribution of their size. There is an empirical literature on this (Cabral and Mata, 2003; Bartelsman *et al*, 2004), which is already rich descriptively and promises to uncover important causal processes. Yet in textbook economics the usual practice is for models to ***assume*** a particular market structure as an ideal type, such as perfect competition, typically starting with a word such as “suppose” or “consider”. This use of modelling language, where a scientist would use empirically established information, treats the assumption as primary; even if evidence is given a role it is treated as secondary (see discussion below on the place of modelling). Ideal-type modelling may have a role in elucidating the consequences of e.g. different market structures, but only if it illuminates what is observed empirically. This position differs from the oft-encountered reactive argument that perfect competition rarely occurs in practice.

A third topic is the rate of return on capital. It has long been known that the distribution is continuous and rather broad, including a small proportion of firms having a negative value (in the short term)(Farjoun and Machover, 1983). This contrasts sharply with the neoclassical assumption (also found in some heterodox accounts) of a uniform rate of profit equal to the standard rate of return – an assumption that implies that firms cannot fail, which is obviously false. Such an assumption needs to be used in very limited circumstances, and with great care.

One major failure to question core theoretical foundations is exemplified by Schumpeter’s important insight that capitalism is a process of “creative destruction”, whereby firms and industries rise and fall in a turbulent manner – very different from the neoclassical model of convergence towards a static equilibrium position (Schumpeter, 1980 [1911]; Schumpeter, 1992 [1942]). His work, although widely seen as a radical break from neoclassical economics, explicitly retained the conventional vision of a static economy (“circular flow” in his terminology), merely adding entrepreneurial “new combinations”, and the resulting turbulence, on top. This resulted in a dualistic model of the economy, seen as composed of neoclassical-like and entrepreneurial sectors. However, this would predict a semi-continuous distribution of the rate of return on capital (a cluster of observations at “zero”, the standard rate, plus a separate positive distribution). As already noted, the distribution is continuous, and it has no peak at the standard rate of return.

Schumpeter’s conception of capitalist dynamism therefore conflicts with a basic observation. However, rather than abandoning the conventional static theory altogether and replacing it with a new one that is compatible with the evidence and with Schumpeter’s own description, the traditional view was merely ***supplemented*** with an additional element. The possibility that the basic theory needed to be completely re-thought was apparently not entertained. This could be called ***incremental mode***, and is related to reactive mode in that they are both oriented to existing theory, respectively attacking or supplementing it. Incremental mode is justified when the existing theory is sound but incomplete, but not when it is fundamentally inaccurate, as here in predicting a cluster at zero profit. In such a situation the selective replacement of its poorly-functioning elements is needed.

Schumpeter’s important ideas have become influential in recent decades, more than half a century after their first expression, for example in endogenous growth theory. Here again it has been done in an incremental fashion, starting from the neoclassical growth model of Solow, then adding some

Schumpeterian features to the model (Aghion and Howitt, 1998). For example, some form of externality is introduced into the conventional aggregate production function model, over-riding the alleged diminishing returns that were a feature of the original. In fact Solow himself has pointed out that such models require the restrictive assumption that the externality is just sufficient to balance the diminishing returns (Solow, 2000). Thus Schumpeter's reluctance to let go of old theory that contradicted his observational analysis is reproduced in this more formal context. The structure is the same: a core that predicts convergence to a static equilibrium, plus an additional component with the role of overcoming that in order to predict growth.

Another example relates to a classic paper by Ronald Coase. In 1937, he observed that "the distinguishing mark of the firm is the supersession of the price mechanism", in other words that within firms people do not trade with one another, rather the coordination is done by means other than price[i]. He then asked, given the efficiency of the price mechanism, "why a firm emerges at all in a specialized exchange economy" (Coase, 1937). Mainstream economic theory, then and now, would predict that the economy would be composed of individual traders, this being the most efficient arrangement. It is remarkable that he had to point this out, and even more remarkable that the question has been largely ignored in the textbook version of the theory of the firm, which ignores the main characteristics that distinguish it from a sole trader, and treats it as if it were an individual.

Firms not only exist, they have become the dominant type of economic organisation. The glaring contrast between theory and reality makes this one of the most important questions in economics. What answer did Coase himself give to his own question? He considered four possible explanations:

- people prefer to work for a master
- those who wish to direct a firm would accept lower pay
- consumers prefer to buy from firms
- market transactions have a cost, e.g. relating to contracts.

He dismissed the first three, and decided that the fourth must therefore be correct. This insight has in fact been fruitful, in that it has given rise to a whole school of research based on the idea of transaction costs, which are far from trivial. But they do not solve the problem: a substantial body of research has tested how well transaction costs explain the boundaries of particular firms – for example, the decision whether to make a component in-house or to buy it from a supplier, and more importantly, the effect of this decision on firm performance. Its success in this has proved to be patchy (Poppo and Zenger, 1998; David and Han, 2004; Carter and Hodgson, 2006). In addition, the transaction costs approach faces special challenges in explaining innovation and the entrepreneurial firm (Hodgson, 2007), which are central to studying growth.

If this concept has only limited success in accounting for the boundaries of firms, it is *a fortiori* difficult to accept it as an important part of the answer to the original question, why firms exist ***at all***. By accepting the necessary truth of conventional market theory, and merely adding another element in the spirit of incremental mode, Coase failed to consider that other explanations might be possible. The real answer appears rather to be that firms exist – and are ubiquitous in the modern world – because systems dominated by them ("capitalist" real economies) vastly outperform all other types of system. According to this viewpoint, the source of capitalist growth is the combination of market trading between firms with non-market coordination within them; crucially, firms have not only the incentive but also the capacity to reduce production costs, and more generally to radically reorganise production (Joffe, *in revision*). A further issue is that Coase framed his question in an ahistoric manner, as a functional statement, rather than as a causal account in historical time. This is discussed further below.

A strategy for systematic description in economics

Could economic theory learn from the practice of biology? It is true that the workings of the body are easier to study, because a physical basis exists that can be examined using microscopes, chemical methods, etc. Even more important, it has a high degree of regularity, so that it is meaningful to speak of "the" kangaroo pouch or "the" human pancreas. Evolutionary biology, however, does not have these advantages; even though it has a known mechanistic basis, this only enables explanations of past evolutionary change to be made, not predictions of the future. In this respect economics is in a somewhat similar position. However, there is no need to be pessimistic about the possibility of systematic description in such circumstances.

The key is that while evolution is open-ended in respect of individual "events", certain patterns recur. It is common to find convergent evolution, in which apparently similar features have evolved independently. This can be whole animals, which have similar features and inhabit similar ecological niches, as in the case of the ostrich and the rhea, which look almost identical but evolved respectively in the old world and the new world. Similarly, three types of anteaters have evolved, in Australia, Africa and America, with similar features adapted to a diet of ants. Or it can be a major feature, such as the wings of bats and birds, which evolved separately from the vertebrate forelimb. Some such features can be subtle, if vital: fish in the arctic and in the antarctic are protected from freezing of their body water by molecules that have an antifreeze action, but which are different in the two cases. Such examples are all based on careful description, not experimentation. Observations of this kind have led Dawkins (2005) to ask the question, "Is evolution predictable?" (see also Dawkins, 2004).

Moreover, the theory of evolution itself is an example of convergent evolution: Darwin was provoked into publishing *On the Origin of Species* by the realisation that Wallace had similar ideas; if Darwin had not existed, the theory and all its ramifications would doubtless have been discovered, it would just have taken longer to elaborate fully.

The reason that this is important for economics is that by analogy it suggests the possibility of using economic history as a source of the type of systematic description that mainstream economic theory has tended to lack[ii]. If a particular train of events has occurred once or even twice, this could be dismissed as case-study material that is attributable to particular individuals or decisions, or to chance. But if something similar recurs many times then there must be some consistent force bringing about this repetition, as long as it is due neither to imitation nor to direct influence such as could occur for example with foreign direct investment. It might even be possible to calculate the probability of a collection of parallel stories being true by chance, for use as the equivalent of a p-value in statistical work. The historical methods used to collect such stories could themselves include narrative and/or statistical techniques, including but not limited to econometric analysis.

A set of observations that emerges from this process provides an empirical generalisation that can then become the starting point for understanding what structures or mechanisms could be capable of generating it. Another way of expressing this is that it uncovers a regularity at a deep level, as contrasted with the search for event regularities which Lawson (2003) has criticised. The corollary is that once a set of parallel stories has been assembled, it then becomes possible to examine differences between them as well, which can also be highly informative.

An example of good practice here is the work of the economist Peter Lindert. In a magisterial and forensic study of the growth of social spending in the past 200 years, the convergent and divergent forces are elegantly dissected (Lindert, 2004). He shows how social spending increased in all economically dynamic societies, tracing its origin to the political circumstances of each rather than to imitation, and that its characteristics differed from society to society. For example, poor relief was a major feature in England and Wales, whereas education was given priority in Germany and in the United States. (He also demonstrates that social spending had no restrictive effect on economic growth, contrary to the expectations of the standard model.)

A similarly repetitive pattern can be seen in the type of sustained *per capita* growth that has occurred in some economies but not others during the last two hundred years, but was unknown before that. Starting in Britain, this pattern was next seen in continental western Europe plus the United States and other "European offshoots". These were followed by Japan and subsequently other countries in East and South Asia, initially on the basis of importation of technology and (sometimes) capital, but then becoming self-sustaining after a few decades. These are often grouped together as capitalist economies (Baumol, 2002), and as with Lindert's study, the convergence is accompanied by sharp divergences, sometimes called "varieties of capitalism". In contrast, technology imports into other countries led to an initial industrial/modernisation process, but sustained dynamism did not follow; these include less successful capitalist economies e.g. in much of Latin America, as well as economies run on communist principles such as the USSR.

Often the onset of sustained growth in the various Asian economies followed economic reforms, generally informed by a type of economics very different from what textbook theory would recommend. The empirical literature on these experiences has raised many important issues, notably on the role of the state (Amsden, 1989; Wade, 1990; Westphal, 1990). The dazzling success of the developmental state that deliberately distorted the free market, e.g. by inducing the “wrong” relative prices (Amsden, 1989), is a direct challenge to core textbook theory on the nature of markets. Twenty years on, the textbooks have responded by ignoring the issue. Furthermore, during much of this period international economic policy has been dominated by the Washington Consensus, which is based on the same traditional flawed view of markets. The belief in the superiority of private ownership is also undermined by the stellar growth performance of China and Vietnam while they retained substantial publicly-owned industrial sectors, but after capitalist-style reforms of the real economy (Qian, 2003; Pritchett, 2003). Clearly the “capitalist” element that makes for sustained growth is different in important ways from the “free market” as portrayed by orthodox economics (Joffe, *in revision*). Yet they continue to be confused (Baumol, 2002).

The place of modelling

Modelling is used both in biology and in economics, but its role is different. In biology, it is mainly used to model causal processes that have been established by other means. It is unusual to find a mathematical account that has no basis in causal understanding, as sometimes happens in physics (Cartwright, 1994), and is rife in economics. Mainstream economics goes further, by relying on models that are tautologically true: they are consistent internally and with the assumptions that are used as inputs, but have no point of contact with evidence. Corroboration or falsification by empirical information is impossible.

Biological models include the Hardy-Weinberg equilibrium formula in population genetics; single- and multiple-hit models of carcinogenesis; the physics underlying the control of muscular movement; the kinetics of biochemical reactions; the sequence of chemical reactions leading to energy generation known as the Krebs cycle; air dispersion models of biologically-relevant pollutants, which can include chemical reactions (formation of ozone); and statistical models of air pollution and mortality.

An instructive example is the law of independent segregation, also known as Mendel’s second law. This states that when two different genes are transmitted from parent to child, they assort independently of each other, so that a random combination ends up being passed on. The basis for this is the mechanical process of meiosis, the process of cell division that leads to the formation of sperm and egg cells. It would be possible to model this law in the style of economics, as a general relationship. However, this would miss the fact that sometimes segregation is ***not*** independent, because when the genes are on the same chromosome they tend to be passed on together. This is known as linkage. Furthermore, the closer they are to each other on the chromosome, the more likely they are to stay together in meiosis; if they are far apart there is a chance that they will be separated by crossing over of the chromosomes. So these exceptions to the law of independent segregation in turn lead not only to the concept of linkage, but also to a classical piece of evidence for locating genes on particular chromosomes, which in due course has led to modern genetics.

In economics, the role of modelling is traditionally different from in the natural sciences. There is rarely a separate explicit concept of a causal explanation preceding modelling as there is in biology. Economists generally present a model with a plausible “story”. The model is not totally arbitrary, but neither is it solidly rooted in causal understanding. It is admittedly more difficult to establish causal relations in economics than in biology, but that does not explain why the ***ideal*** is to base models on axioms, even when empirically unsupported, rather than on reality.

Accordingly, tractability, parsimony and elegance are the primary criteria by which models are judged, rather than empirical adequacy. This tends to be true even in models that are explicitly intended as the basis for statistical testing, but is at its most extreme in textbook theory. Simplicity is treated as if it were an end in itself, whereas a biologist would see it as secondary to the main task of constructing a model that corresponds to descriptive and causal reality. As economists are fond of saying, simplicity is positive when a simple model captures the essence of a situation. The problem arises with models that fail to capture the essence, or even distort and obscure it.

Distortion is quite distinct from simplification, but many economists appear not to realise this. The classic example of simplification is the application of Newton’s laws of motion to a projectile in the Earth’s atmosphere. The basic model ignores air resistance, and is therefore only an approximation.

This can readily be remedied by introducing air viscosity into the model – an example of incremental mode. In contrast, ***to say a model is distorted means that it cannot be remedied in this way***. Under these conditions, incremental mode will not solve the problem.

An important example, central to the neoclassical theory of the firm and ubiquitous in textbooks, concerns the relationship of costs to the scale of production. The standard model is that as the scale of production rises, beyond a certain point the average total costs begin to increase, producing a U-shaped curve. (Strictly speaking the theory concerns firms' *perceptions* of the cost curve, rather than objective reality, but the implication must also be that the true cost structure is the same, given the standard neoclassical assumption of perfect information.) Textbooks differ in how they present the U shape, in particular whether they attempt to justify it. Many just present it without any rationale, perhaps with the word "typical" attached, presumably to deflect students' attempts at critical thinking. The better ones at least give examples and a plausible rationale.

The problem is that the classic U shape appears to be wrong, at least for a large proportion of firms. In industry, costs are usually stable or fall with scale, rather than rising. Empirical research, based on taking a sample of firms and asking the appropriate person within each of them about their perceived cost structure, indicates that it applies to rather a small proportion, variously estimated at between 5 and 11 percent, with the first paper having been published in 1952 (Eiteman and Guthrie, 1952; Scherer and Ross, 1990; Blinder *et al*, 1998). So although the textbook account may sometimes apply, its presentation as a general description is false.

Unfortunately, the 1950s debate became deflected away from the observation that the standard account is empirically wrong. This was because the conclusion of Eiteman and Guthrie's paper was in reactive mode, "that … theory should be revised in the light of reality". The issue became whether or not the findings invalidated marginalist theory, which is a bad choice of ground given that the theory is tautologously true. To dissipate the force of the criticism, the defenders of orthodoxy merely had to assert the internal coherence of their theory even without the U-shaped curve, which was portrayed as not central to it ("merely pretty pictures")(Ritter *et al*, 1953). The battle was fought on their territory, the detailed specification of the theory, rather than on the scientific grounds that deliberately spreading an untruth is wrong. And the "pretty pictures" are still there several decades later.

If the practice in economics were to give prominence to description, such falsehood would be impossible. It survives as a standard model because it fits with the requirements of neoclassical theory, not because it has any relationship to reality. At least for this example, the closer one gets to the theoretical core of textbook-style economics, the more distorted is the analysis of how the economy works. This is unique among academic disciplines.

The basis of the U shape is a thought experiment, combining one fixed and one variable factor of production, which is a defensible procedure. However it raises an important further question: whether the firm's decision making process that is being modelled actually occurs in real firms. The answer is no, firms do not recognise it as a description of what they do (Lee, 1998). Thus the model also fails to correspond in respect of mechanism.

Certain textbooks go further than merely presenting as universal a model that is empirically false in most instances. An extreme example is "Intermediate Microeconomics" by Varian (1987 to 2010), which is widely used. The following situation is postulated:

> "Suppose that a firm has chosen a long-run profit-maximizing output … [with] constant returns to scale and that it is making positive profits in equilibrium. Then consider what would happen if it doubled the level of its input usage. According to the constant returns to scale hypothesis, it would double its output level. … its profits would also double. But this contradicts the original assumption that its original choice was profit maximizing! We derived this contradiction by assuming that the original profit level was positive; if the original level were zero there would be no problem: two times zero is still zero. This argument shows that the only reasonable long-run level of profits for a competitive firm that has constant returns to scale at all levels of output is a zero level of profits[iii]." (emphasis in the original)

Everything here hinges on assumptions. This is not the familiar criticism of economic theory that its assumptions are unrealistic – that would merely be a reactive response. ***It is giving assumptions the role that evidence should have***. The entire sequence of reasoning is concerned only with the set-up of the thought experiment and reality has no role, but this does not prevent a conclusion being drawn that appears to be about real-world profits for a firm under conditions of perfect competition. Some might see this as a paradigm case of the relationship of textbook economic theory to the real world: the substance is merely an artificial construction, but an inference is drawn that makes apparent reference to reality.

This example may seem extreme, but it is not unique. The following section has:

> "When a profit-maximizing firm makes its choice of inputs and outputs it reveals two things: first, that the inputs and outputs used represent a *feasible* production plan, and second, that these choices are more profitable than other feasible choices that the firm could have made … This equation is our final result. It … comes solely from the definition of profit maximization. Yet it contains all of the comparative statics results about profit-maximizing choices!" (emphasis in the original)(Varian, 1987 to 2010)

The definition of feasibility here is intriguing: a production plan is feasible if it is chosen by a firm that we have brought into existence for our thought experiment. And we know that it is the most profitable such plan because we have assumed the firm to be profit maximising. From this a series of statements is made as if they had the backing of some reality.

These two passages are (admittedly extreme) examples of theories that are tautologically true. They cannot be confirmed or refuted; evidence has no role. That this textbook is now in its eighth edition, and is widely recommended, says a great deal not only about the approach of its author, but of the mainstream of the economics profession more generally. As far as I know, economists have not criticised the strangeness of reasoning in these two passages.

Boundary compatibility – relationship with knowledge in neighbouring disciplines

The relationship between the different natural sciences is one of complementarity. They fit together neatly. For example, in considering the toxicity of an environmental chemical, a biologist has to consider the chemical aspects of the problem as well as the biology. There is not, and cannot be, any conflict between them. Indeed, the relationship is stronger than just compatibility: most biological research is devoted to finding chemical mediating pathways that can explain how a particular biological system functions. Physics also makes an important contribution to the understanding of biological systems, for example analysis of the mechanical forces in the heart and circulatory system. Physico-chemistry is the ***mechanistic*** language of biology. There is also an ***emergent*** language which is specifically biological; the study of life cannot be reduced solely to its physico-chemical elements.

The situation is different in the social sciences, and especially in economics. Neoclassical economics seeks an individual-level (mechanistic) account, yet is famously dismissive of psychological reality in formulating its models: one of the traditional criticisms is of the assumption of rationality. This can lead to a particular variant of incremental mode, in which behaviour is first depicted as rational, then the discrepancy of actual behaviour from this construct is added in. A double error is thereby created: the original distorted model, and the discrepancy that owes its existence to it. A preferable approach is directly to study the behaviour in its own right, e.g. as an evolved heuristic that the brain actually uses.

The most important contributors to the critique of rationality in traditional economic theory are HA Simon (1976), who coined the term "bounded rationality" to contrast with the conventional assumption of perfect rationality, and Tversky and Kahneman (1981). The limitations of human calculating ability were also stressed by von Hayek (1937).

Change is underway in this respect, largely resulting from the influence of Tversky and Kahneman. The sub-discipline of behavioural economics is thriving, and is now regarded as mainstream (Smith, 2008). This move towards boundary compatibility is undoubtedly constructive. Currently there is tension, which is potentially creative, between this more realistic account of behaviour and the tradition of parsimonious modelling with unrealistic psychological assumptions.

Rationality is not the only issue, only the most prominent of them. Neoclassical theory also traditionally assumes perfect information/knowledge, which has been thoroughly explored by information economics. A third assumption is perfect foresight, which is especially important in respect of causality, because it relegates time and causation to an automatic extension of the agent's motivation. A theory that does this cannot encompass unintended consequences, with dire results. Indeed the term "agent" here is inaccurate, as the absence of uncertainty and the other elements that enter into human decision-making remove agency from the scene, transforming it into an automatically realised optimum.

Criticisms of the assumption of perfect foresight have been around for many decades (Knight, 1985 [1921]; Keynes, 1937; Hayek, 1937), largely in reactive mode in relation e.g. to equilibrium analysis and the analysis of risk; nevertheless they remain relevant today, because the assumption still underlies much current theory (Keen, 2001). One of the dire results is that risk becomes reduced to a probability distribution, but one with known parameters, radical uncertainty being excluded (Keen, 2001). The incorporation of such a perspective into apparently sophisticated financial models in the last few decades is a large part of the reason why the financial sector collapsed in 2008. This should have been foreseen because a smaller version of the same type of crash occurred ten years earlier with the demise of Long Term Capital Management, a company that boasted the most brilliant brains in economic finance, including Nobel laureates (Lowenstein, 2002). Theoretical weaknesses have real consequences.

Another far-reaching outcome is that the theory is impoverished by its inability to build unintended consequences into the conceptual model of how the economy works. This is deeply ironic, as the cornerstone of modern economics is Smith's "hidden hand" underlying the price mechanism, which is basically an account of unintended consequences at the aggregate level of the market. It is a simple system of balancing (negative) feedback, and it is this that creates the endogenous causal processes that lead to the balancing of supply and demand. It thereby generates an emergent property – the language is now economics not psychology[iv].

The tendency towards a stable equilibrium dominates mainstream economics, but it fails to account for key economic phenomena such as bubbles and capitalist growth. However, the same conceptual structure of unintended consequences, endogenous causation and emergence is not limited to such convergent systems. For example, when applied to an economy dominated by firms, unintended consequences at the aggregate level have been shown to explain why successful capitalist economies tend to grow inexorably (Joffe, *in revision*).

A deeper problem is therefore the perception that if only economic theory would adopt a more realistic view of human nature, it would develop a good model of the economy. Partly this results from the lack of realisation that any social structure, including an economy, is not simply the summation of the actions of all its included individuals. The price mechanism, and capitalist growth, are not only unintended consequences, they also importantly operate at the aggregate level of the market or sector. The development of institutions is an example of this type of emergence, which Vernon Smith (2008, following Hayek) terms "ecological rationality". Not all social phenomena are the result of conscious design (Hodgson and Knudsen, 2006).

The traditional reactive criticism of neoclassical economics, that behaviour is not rational in the real world, completely misses this. Reactive mode has meant a decades-long and still-continuing confrontation between proponents and opponents of rationality, which has obscured the fact that important aspects of economic life are not reducible to behaviour ***at all***. These would not be adequately addressed by an incremental solution that substitutes new decision rules for old. Another way of expressing this is: economic phenomena are partly the result of behaviour, but there is also an emergent level of specifically economic relations. They cannot be reduced solely to psychology, however accurate.

Causal explanation: mechanism and emergence

In addition to compatibility with neighbouring disciplines, a natural scientist would generally expect all three major components of a theory, model or hypothesis – evidence and assumptions (input), mechanism, and predictions (output) – to be compatible with existing knowledge. There are of course exceptions to this, and they are famous not least because they are exceptions. For example in physics, quantum electrodynamics theory has no plausible causal mechanism and yet is able to make predictions that are accurate to eleven significant figures (Feynman, 1990). Such exceptions tend to occur in fundamental physics, where the scale is unimaginably small or large. Sciences (including physics) that deal with more familiar-scale reality tend to have this three-fold compatibility.

For example, an important question in neurophysiology during the 1920s and 1930s was how impulses are relayed from nerves to muscles through the neuro-muscular junction, so enabling control of posture and movement. The focus was on the mechanism by which this occurs. The inputs – observations rather than assumptions – were taken as common ground, the big controversy being electricity versus chemistry: is transmission an electric current (electrons) crossing the space between the nerve and muscle fibres? Or is there a chemical mediator? This was solved by measuring the time that elapses between the firing of the two types of fibre, which was far too long to be an electric current.

If a chemical mechanism was involved, this would predict that its apparatus was present in the nerve and muscle fibres. This prediction was then confirmed by other methods, e.g. visualising vesicles of the transmitter in the nerve fibre ending using the new technique of electron microscopy, chemically analysing the substance and finding it to be acetylcholine, finding the enzymes that synthesise it, and reproducing the effect on the muscle fibre by introducing acetylcholine instead of stimulating the nerve. Interestingly, all except the last of these is an observation not an experiment[v].

This is typical of how biology progresses. What starts off as two rival hypotheses becomes a description of the underlying process, in terms of the key components of the system and their properties or capacities (Cartwright, 1994). Mechanistic explanation is description at a deeper level, and economics typically seeks an equivalent when relying on individual behaviour, rational or not, to account for economic phenomena. The alternative to a reductive account based on the deeper level is in terms of emergent properties.

Convergent economic history could provide a starting point for seeking causal understanding of either type. An example is the historical record outlined above, that appears to show that under certain conditions, a transition occurs after which a particular economy experiences sustained *per capita* growth. The most likely explanation is that some ***shared*** feature of the capitalist system has the potential to cause this, despite capitalism's variety, for example an institutional change (Joffe, *in revision*). A more conventional view, based on extrapolation of human action directly to consequences, is Schumpeter's view that entrepreneurs' innovations are responsible for capitalist dynamism. But if this were true, one would have to ask: why are such innovations more frequent and/or more successful in these economies? (Joffe, 2010)(Schumpeter (1992 [1942]) himself explicitly denied the capitalist specificity.) Any explanation in terms of individual behaviour needs to be able to answer such questions.

The methodological issue is that it is likely to be an ***endogenous*** feature of the economic system. If it were an exogenous force, such as Schumpeter's entrepreneurial innovation and/or technological change as in the Solow model, this would have to have been constant in magnitude to explain e.g. why growth of the US economy has been so close to exponential over a period of two hundred years.

One way of analysing endogenous causal processes is to use system dynamics. As Forrester pointed out, specifying the causal processes operating in complicated systems, and especially their feedback loops, illuminates the endogenous causality involved (Forrester, 1970; Lane, 2007). It then becomes possible to model how the feature of interest, growth in this case, is generated by the structure of the system, rather than having to portray it as a response to a shock, disturbance, or other outside influence. Even where an exogenous cause or shock is involved, it is necessary to examine its effects on the endogenous causal processes of the system – just as one can only understand the effect of burning on human skin if one has a good account of how skin tissue operates and how it repairs itself. Rather than having to rely on exogenous causes such as behaviour, or scientific/technological innovation, or resources (Pomeranz, 2000), this systems approach thus allows the analysis of endogenous causation and of emergence, bringing economics back into economics.

Returning to the behavioural assumptions underlying economic theory, if the theory gave good predictions, would it matter that the proposed mechanism were incompatible with the known properties of human behaviour? It could be, for example, that lack of rationality merely results in noise, so that the assumption of rationality produces a good estimate of the central tendency. The vagaries of observed individual behaviour would then be like a statistical distribution around the mean. For a scientist, this would be a step forward in achieving the prediction, but also a source of frustration that the real mechanism is obscure, as in the quantum electrodynamics example.

A contrary view was put forward by Friedman (1953) in an influential paper. He stated that assumptions can be justified, however unrealistic they appear, as long as they give good predictions. The sub-text to this was that he was defending mainstream economics from the standard reactive criticism of unrealistic assumptions. That the paper is still clearly relevant today is indicated by the publication of a recent book with the same title (Mäki, 2009a), that reproduces it as "the classical essay in twentieth century economic methodology". The book shows that its interpretation is highly contested. While it is true that not everybody shares Friedman's views on methodology, in Mäki's view the "popular legacy" of this article has influenced research presentations as well as textbooks, e.g. in stating that assumptions can be unrealistic, and that "as-if" explanations are valid (Mäki, 2009b). I will just comment on two examples that directly involve biology.

One example given was of an expert billiard player, the aim being to predict the shots. The point was that Newtonian mechanics would enable the calculations to be made, even though the player did not actually use Newton's equations while playing. Friedman considered that this was a good analogy for what economists do when their equations describe economic behaviour in terms that the economic agent would not recognise. The model describes the behaviour "as if" it were a calculation, while acknowledging that the actual neurophysiological processes do not correspond to the description. In philosophical terms, this is an instrumentalist view whereas natural scientists set out to provide a realist account of mechanism (although some dispute this interpretation (Mäki, 2009b)).

More importantly, Friedman's account is scientifically impoverished. It seeks a model that will predict the shots[vi] (events), whereas a biologist would aim for a ***causal theory of mechanism***. Elements could then be modelled mathematically. For example, a neuro-physiologist would seek to explain how expertise in billiard playing is developed and executed in terms of neuronal pathways and neurotransmitters. The historical success of biology has been the result of posing and solving such causal questions.

The practice of theoretical economics has been largely based on modelling without causal understanding, and this affects even those economists who do not share Friedman's methodological views. (This may be why economics has such a poor record in prediction.) The serious consequence is that important features of reality are missed; whereas a model needs to be rather simple to be useful, a causal theory can explore connections of a widely differing type – especially important in the complex reality that economics deals with. The reduction of theory to modelling could be termed "model dependence". As *The Economist* magazine (2009) stated in relation to the financial crisis:

> "By assuming that capital markets worked perfectly, macroeconomists were largely able to ignore the economy's financial plumbing. But models that ignored finance had little chance of spotting a calamity that stemmed from it."

Models need to be embedded in broader causal theories. This enables the overarching theory to guide the use of a component model for a specific purpose. If this is not done, the alternative to dependence on a single type of model, with no possibility of systematic thought outside it, is to have a range of models – with the choice of which to use being left merely to intuitive judgment.

A second example given by Friedman is pure biology, so is directly relevant to the analogy being explored in this paper. He considers the leaves on a tree, which are distributed "as if" designed to catch the maximum sunlight. It is true that the observation about the distribution of the leaves is not trivial. But a biologist would go further and try to ***explain*** this distribution. For a situation like this there is a ready-made explanatory framework in the theory of natural selection: the trees with a genetic tendency towards the most beneficial leaf arrangement are more likely than others to survive and pass on their genes. The biologist's task would be to identify the particular pathways in the general framework, for example, what competitive pressures are acting on the tree, perhaps in terms of climate or of a changing environment; identifying the development process and relevant genes, establishing the biochemical pathways by which they act; and so on. The idea that an "as if" explanation is satisfactory would be difficult to comprehend.

Methodologically, Friedman's statement is functional: the distribution of leaves fulfils the need of the tree to capture maximal sunlight. The problems of such potentially teleological arguments are well recognised by scientists, and in the philosophy of science (Stanford Encyclopedia of Philosophy, n.d.). Philosophers of biology have long realised that they can readily be overcome ***in the biological context***, because it is straightforward to make a "translation of talk of functions into terms of talk of adaptations", i.e. a causal one based on differential survival and reproduction (Ruse, 1973); it then also becomes a historical account in real time. Outside biology such a translation cannot in general be made; a functional statement may not correspond to any real causal processes.

When Coase tried to explain why firms exist, his conclusion was phrased in functional terms, which comes naturally to mainstream economists, nowadays as it did in 1937: firms are said to exist because they fulfil the need to reduce transaction costs. It posits a reality which is nicely interconnected ***at any given moment***: if firms exist, it must be because they have current cost advantages. Such an approach excludes the possibility that the causal mechanism operates in ***an open-ended fashion across real historical time***: e.g. that firms exist – and prosper – because their control over production enables them to compete more successfully than sole traders (Joffe, *in revision*). This is an argument in terms of differential consequences[vii].

In contrast, a biologist (or other scientist) would start by observing how firms come into being. Generally, a firm exists because it is set up by someone with initiative who either has resources or can obtain them; this creates an authority structure which normally then persists. Coase's question is still relevant, but it becomes, "if trading between individuals is more efficient, why do firms not disintegrate?". This leads to examining the type of firm that does break up, e.g. because in a high-tech context the "workers" are themselves at the cutting edge of innovation, and they therefore have both the opportunity and the motivation to establish their own firms. In the general case, however, firms do not break up in this way, and show great capacity for persistence. A biological equivalent of Coase's explanation would be if a biologist tried to explain multi-cellular organisms, and their constituent organs and systems, in terms of the coming together of different types of cells. This would seriously harm embryology!

Thus, scientific methodology aligns the theoretical account with a process occurring in historical time. It is phrased in terms of consequences, not of neatly fitting together. It has been highly successful in biology.

Some of these issues have direct relevance to economic research, for example the impact of a changing business environment on the survival probability of firms in different sectors, and the tracing out of mediating causal pathways (Foster *et al*, 2008). Oddly, Friedman's view is that such pathways do not really matter. Thus he defends the assumption that firms maximise profit as being justified, irrespective of whether it is brought about by the ***motivation*** of the manager to maximise profits (behaviour – the standard neoclassical assumption) or alternatively by the ***differential survival*** of firms such that those that maximise are more likely to survive (consequences). It is not just that it may be difficult to make this distinction empirically, which is true, but that they are regarded as ***equivalent*** – causal mechanism does not matter. It is actually an important practical point, because the widespread perception that the private sector performs better than the public sector could be justified if managers' motivations were the operative factor. But if it is merely that the private sector is better at getting rid of poor performers, then this would be a bad argument for private sector involvement in running, say, a city's hospital or a small town's school, where ceasing to exist is not an option.

Friedman's conclusion is that assumptions can be justified, however unrealistic they appear, as long as they give good predictions. Strangely, he does not provide evidence for predictive success of any mainstream economic theories, apparently regarding it as self-evident that they must be correct. This is the most disturbing part of the essay, from a scientific viewpoint: a lack of exposure to empirical evidence. The theory is deemed to be true by definition – tautologically true. Lest this be seen as an over-reaction, recall that mainstream textbooks all portray the cost curves of firms as universally having a shape that is in fact uncommon. This type of methodology leads to a hermetically sealed, circular conception of the world, structurally similar to theology, in which any accurate relationship to reality is accidental. Whereas scientists set out to make ontological statements, all too often economists are imprisoned in epistemology.

Conclusions

Mainstream core economic theory may superficially resemble some types of natural science, but it behaves as a set of mathematical modelling techniques rather than as a science. This is reflected in the ideal of basing core theory on axioms, even if empirically unsupported, and in the still-widespread practice of producing simple, elegant models based on assumptions rather than evidence. Simple models are useful ***if*** they capture the essence of a situation but not if they distort it. In a science like biology, a prior stage is to generate a broader theory, a causal understanding of the possibly multiple influences involved in the situation under study, based on systematic description. This helps to prevent both distortion and the risk of tunnel vision as occurred with recent models in macroeconomics that ignored finance.

One practical way of achieving a sound empirical basis would be to seek examples of convergent historical processes, analogous to convergent evolution in biology, and to examine both the similarities and the divergences between the different instances. More generally, a scientific economics would pay greater attention to causal accounts where the assumptions, mechanism and outcome are all compatible with the empirical evidence.

Much current economic research does aim at an empirical basis and a broad causal understanding. The issue is what to do about the residue of unscientific practices – not only in specific research projects, but particularly in integrative activities such as textbooks and teaching, and in methodological writings. This would mean letting go of some traditional items such as:

- believing that markets are all alike, and all have the property of self-regulation;
- focusing on convergence and stability while ignoring divergent forces and bubbles;
- reducing economics to behaviour;
- seeking “to predict the shots” (events) rather than to uncover the underlying causal explanation;
- obscuring the open-endedness of historical time by the assumption of perfect foresight;
- mistaking functional or “as-if” statements for causal explanations;
- and especially, reproducing apparently “factual” statements that are in fact wrong.

A scientific economics would abandon reactive mode and limit the use of incremental mode, removing the aspects of theory that are not based on evidence, and replacing them with a well-founded empirical basis. This could be called selective replacement mode. It would then look like the best practice in current/recent research, organised around a core that is worthy of it. There is some distance to go.

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[i] In this context, a “firm” indicates an organisation that employs wage labour, as contrasted with a self-employed worker.

[ii] Other sources of empirical information are also potentially useful as a basis for theory, for example econometric studies of structural changes – see e.g. Summers (1991) and Juselius (2010) An advantage of comparative economic history is the magnitude of the contrast between, say, Ghana and South Korea; or, South Korea in 1960 and 1990: the contrasts are sufficiently great that they outweigh any uncertainties of data and statistical method.

[iii] Note that “a competitive firm” here does not mean a firm that is good at competing, but rather a firm in a sector characterised by perfect competition; and that the term “profits” does not include return on capital, which is non-zero.

[iv] The relationship of systems containing feedback with endogenous causal processes and with emergent properties is discussed below.

[v] Also, only the first one is measurement, so that the statement “Science is measurement” (Cartwright, 1994) is false.

[vi] In Lawson’s terms, “event regularities” – see Lawson (2003).

[vii] It is true that Coase’s view could also be expressed in this way, rather than in an ahistoric form, but it would not be the only such explanation. Evidence would then be the deciding factor.

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