WHAT'S WRONG WITH ECONOMICS?

A PRIMER FOR THE PERPLEXED

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WHY METHODOLOGY?

> A man is not likely to be a good economist if he is nothing else.
>
> John Stuart Mill[1]

The need for economists to think about economics became apparent after the global financial crisis of 2007–2008. Few economists predicted the crash; more damningly, few envisaged the possibility that such a collapse could occur, any more than the crash of an algorithmic system. Students of economics asked: what is the point of studying economics if it can't tell you what is going on, or offer policies to prevent bad things from happening? For what happened was the worst economic crisis since the Second World War. Terms to describe it go from the Lesser Depression to the Great Recession.

The roots of this failure do not lie with the incompetence or inattention of individual economists, but deep within the way economics is done – its methodology. This may sound dry and boring, but the methods of economists are key to understanding how and why economics goes wrong. Neoclassical economics has developed a peculiar method for studying the economy, and the use of any other method is not regarded as economics. In other words, the subject matter of economics is defined by the neoclassical method. Models based on this method allow for only a limited range of possibilities. Events which might occur outside this range are not picked up

on economists' radar screens. Models which show financial markets to be efficient – as most of them did – will not give you the collapse of 2008. The spate of papers offering explanations of the crash came *after* the crash. We now learn that, with a bit of uncertainty, 'multiple equilibria' can be 'endogenously' generated. But there was no 'uncertainty' before the crash, only insurable risk. So, this book aims to discover why the most influential discipline for making public policy is so often cut off from reality.

Economists usually scorn the study of methodology. 'Those who can, do science', said Paul Samuelson (1915–2009), 'Those who can't, prattle on about methodology.'[2] Frank Hahn (1925–2013) similarly claimed, 'I want to advise the young to avoid spending too much time and thought on methodology. As for them learning philosophy, what next?'[3] In other words, these eminent economists didn't see the need for students of economics to think about what they were doing. Their message was not how to think, but what to think.

If economics were a natural science, this would be good advice. Natural scientists don't spend their time agonising about their methodology. They believe, with good reason, that the methods they have evolved for understanding physical matter are adequate for discovering the truth. (In fact, reflections on method have always intertwined with developments in physics from Descartes to Einstein. But for all practical purposes, the methodology of the natural sciences is fixed.) Most economists take the same line. Their world is peopled with human robots and they aim to establish 'laws' about the behaviour of these machine-like creatures. A complete set of laws is not yet to hand: but they will catch up with the natural scientists in the end, perhaps after the neuroscientists have completed their work on the brain. They are loathe to admit that the material they study and try to understand does not behave with the law-like regularity of natural phenomena. Humans are, uniquely, inventive animals. They are aware of who they are, reflect on their experiences, set themselves goals, relate to each other and their environments in complicated ways, puzzle about the morality of their actions, adapt creatively to new situations. By the exercise of their minds and imaginations, they modify the future – their own, and the world's. Their games cannot be 'sussed out'. The most secure laws of economics are tendencies at best.

Open and closed systems

John Maynard Keynes (1883–1946), one of the greatest economists of all time, pointed to the inescapable fact of uncertainty:

> It is as though the fall of the apple to the ground depended on the apple's motives, on whether it is worthwhile falling to the ground, and whether the ground wanted the apple to fall, and on mistaken calculations on the part of the apple as to how far it was from the centre of the earth.[4]

The implications of this statement are profound. Keynes is saying that humans are not 'programmed' to behave like apples. Humans are parts of complex systems, whose motions cannot be explained by the causal laws on which natural science is built.

The difference between natural and human material can be expressed by saying that a *closed* system is one in which 'if X, then Y'-type statements apply, whereas an *open* system is one in which they don't.[5]

True enough, there is a lot of variety in a closed system: in a game of chess, there is a vast number of possible combinations. But the variety is finite, and in time all optimal moves will have been made. (Or so it seems: mathematicians claim that chess is so complicated that potential optimal moves approach the infinite.) The principle of limited variety is true of the physical world. If you roll a fair die, there *is* a ⅙ chance of each outcome. This 'truth' does not depend on how the die views the situation. But if you say that a fall in interest rates by X *will* lead to an increase in investment of Y amount you are converting an open system into a closed system. Only if the rest of the economy is frozen by assumption or decree would a change in X produce a predictable effect on Y.

What economics does is to convert open systems into closed systems by excluding 'moves' which would render the system unstable. Dictators 'freeze the frame' by order: economists do it by 'modelling'. They model the world as a giant computer network in which every possible move has been programmed, and anything 'outside' the frame excluded by assumption. We will have more to say about the freezing technique in Chapters 4 and 5. But even at this point one can assert that their claim to be

able to predict behaviour is greatly exaggerated. Apples do not choose whether or not to fall to the ground, any more than a hurricane chooses whether or not to happen every few years. They have no choice; the task of science is to explain why they behave in the way they do, not why they choose to do what they do. Economists are seduced by the thought that, because humans are part of nature, their code can be cracked just like that of physical objects. But even those who hold out this hope admit that humans are uniquely complicated. This makes social systems for all practical purposes almost infinitely complex.

The method of freezing the frame, and including in it only measurable moves, works well enough in the analysis of individual markets or firms in isolation. But it breaks down when applied to a whole economy. This reminds us that economics has its roots in microeconomics – the study of the logic of choice in a single market without money. Money, the errant or wandering cause, which causes whole economies to misfire, was added as a separate field of study. In the standard textbook it is introduced in later chapters as a 'complicating' factor. Keynesian macroeconomics tried to take this complicating factor into account in explaining economy-wide malfunction. More recently, economics has reverted back to microeconomics, with macroeconomics squeezed out by assuming that money can be got to behave in a non-disturbing way. Microeconomic theory can then be 'scaled up' to explain the behaviour of the whole economy. However, the big questions of the macroeconomy – what causes prosperity or depression, inflation or deflation, growth or stagnation – cannot be satisfactorily answered with the tools of microeconomics.

The method of economics

The study of the methodology of economics is the study of the methods which economists use to gain knowledge, rather than a study of the knowledge they claim to have acquired. That is to say, it is not primarily a study of economic doctrines. Rather, the proliferation of economic doctrines testifies to the failure of the established methods to generate knowledge, if by knowledge we mean *true belief.* The methods which produce 'laws' in physics produce doctrines in economics. The hypotheses of economists are largely untestable. In this they resemble religious beliefs. The question is

not whether economics can be made more like a natural science, but whether different methods might enable it to improve its understanding of human behaviour. The charge is not one of false reasoning, but of reasoning from over-simple premises.

In today's classroom, students are fed models: the better the university, the more complete their drilling in the conventional models. The basic model is that of a perfectly competitive economy, in which prices adjust the preferences of perfectly informed buyers and sellers to each other. Students must be taught to learn such models, not question them. The collapse of the financial system in 2008 took nearly all economists by surprise, because such collapses were 'outside' their models.

Economic models are supposed to be closely related to the real world: once mastered, the model offers reliable knowledge of 'what is going on'. But this relationship is not obvious. Economic models are not like model aeroplanes, which are scaled-down versions of a real aeroplane. It's easy to see if you have a bad model aeroplane – it looks nothing like the real thing. But economic models are not miniaturised replicas of real things. They typically consist of logical deductions from axioms (truths treated as self-evident). How do you know that your economic model has any relation to reality? That the premises of the argument have not excluded parts of reality important to understanding what might happen? A reply might be that the model is a caricature which nevertheless contains the essential features of the real thing. But a caricature is only identified as such because we have an actual face or body to compare it with. Economists, like natural scientists, are committed to bringing their caricatures 'to the data', and rejecting those which are disconfirmed by the data. But I shall argue that no secure tests exist for many models which claim authority. Economics' inability to validate its most important hypotheses empirically means that it has a strong tendency to slide into ideology. The pretence to science makes invisible the rhetorical character of much of its thinking.

Economists suffer from 'physics envy' because they believe that their material – human beings – being rooted in nature, are only more complicated versions of natural objects. Like the technologists, they believe that with enough data and computing power they can 'crack the code' of human behaviour. This quest – and the envy which inspires it – is misplaced. It drives economists further away from the 'real' world of

humans whose behaviour they are trying to understand. They can get closer to the real world by making use of the insights of painting, music, and literature, and, in the narrower sphere of social science, by collaborating with other disciplines like psychology, sociology, politics, and history. Such cooperation will broaden economics' view of what is important and true about human life, without losing the sharpness of its particular angle of vision. These studies ought to be part of the education of an economist because they suggest valid ways of seeing the world which lie outside the frame of mainstream economics. The demand for pluralism is not a demand for a new theory, but a demand for a wider vision, from which new theories (plural) may emerge, applicable to different parts of social life. The historian Eric Hobsbawm looked forward to a terrain of enquiry on which history, economics, and sociology could meet. Add psychology and politics and you have the agenda of this book.

The value of pluralism can be illustrated by the ancient Indian parable of six blind men trying to identify an elephant. One grabs the trunk and thinks it is a snake. Another thinks its flank is a wall, another the tail a rope, another feels an ear is a fan, another still thinks the legs are tree trunks, and the last reckons the tusk to be a spear. The point is that, blind, none can see the whole picture; to do so they must collaborate, share what they have found from their own vantage points, and piece together the elephant from their combined insights. Economists must learn to listen: to those in other disciplines, and to their own dissenters.

The other disciplines do not, of course, speak with single voices, and it is greatly over-simplifying to talk of a 'psychological' or 'sociological' or 'historical' point of view. But they each shed a distinctive light on the topic of human behaviour, which is my justification for giving them separate chapters.

1. *Blind Monks Examining an Elephant* by Itcho Hanabusa, 1888.

So what does the study of economic method involve? Most obviously, it involves philosophy – thinking about the conditions needed for making true statements, and how far these conditions apply to economic propositions. Almost entirely lacking from economics is any explicit argument pertaining to its epistemological status – its status as knowledge. Only a total disregard for philosophy enables economics to claim that it is a positive science, immune from judgments of value.

A key issue is whether logical deduction from tight assumptions is the best way of 'getting at the truth' of the world or whether it is better to pay more diligent attention to the facts even though this might mean using a looser logic. As failure to foresee the crash of 2008 testifies, precision can be purchased at the expense of usefulness. For the purposes of policy, it is important to ask how far, and in what areas, the propositions generated by current methods of doing economics are sufficient pointers to good policy, and where they need to be complemented by understandings gleaned from other ways of studying human behaviour.

Mainstream economics believes social phenomena are best understood as the summed-up behaviour of individuals, an approach known as *methodological individualism*. This method has two characteristics: the only *actors* or *agents* recognised on the economists' social map are persons (this 'realistically' includes households and small firms, but not organisations or classes), and individual choices and decisions are *independent*, that is, specific to those making them. This twofold claim enables economists to use a simple additive formula to demonstrate that aggregate outcomes 'are the result of an enormous number of discretionary decisions by individual actors'.[6] With the further assumption that individual plans are, on average, fulfilled – that is, there is no uncertainty – one can derive an aggregate number simply by adding up the individual plans.

There are two huge flaws in the approach which represents individual choices as parallel straight lines. The first is that explanations in terms of individuals alone omit the relations between them, and thus the social structure in which choices are made. Individuals are part of 'networks' of choice. So aggregate outcomes of any kind are the sum of individual choices plus the social structure. The second flaw is summed up in the phrase 'the fallacy of composition'. Even if made independently, individual choices affect each other. We each decide how much of our income to save. But an increase of $1 in my saving does not increase total saving by $1, because it reduces your income by $1, so if everyone else saves the same proportion of income as before, the total of saving goes down not up. In the words of songwriter Leonard Cohen, 'You can add up the parts, you won't have the sum'. (For further discussion, see Chapter 7.)

For mainstream economists it is not enough simply to specify individual persons as the sole choosing units. Their units choose 'rationally'. They have coherent plans; act purposively to achieve them; and calculate the most efficient means to get what they want. Mainstream economics presents to us one human type – Economic Man or *homo economicus*, the human calculating machine, continually calculating how to get the most ('maximum') gain he can for the least cost. This calculation is done in prices, everyone and everything has a price.

These two methodological rules – the concentration on individuals, and their depiction as calculating machines pure and simple – are the clue to

what goes wrong in mainstream economics. Economists reduce social structures to economic transactions and erect one aspect of human behaviour, calculation of costs ('how much will it cost me to do X rather than Y?'), into a universal law of all human behaviour. They are in a quandary when you point to motives for action like love, devotion, pity, courage, honour, loyalty, ambition, public service, which on any reasonable interpretation are not motivated by subjective calculation of gain or outcome. The codes governing such behaviour may be 'beyond price', because it would be felt shameful to break them. Economists have to say that such motives appear to be irrational, but may be rational in situations of limited information. They are forced by the requirements of their own reasoning to squeeze their explanations of human behaviour into absurdly narrow channels.

This raises a hugely important question which will run through this book. Is the unlovely creature *homo economicus* intended to be a realistic description of a human, an ideal type, or simply a requirement of deductive theory? My own view is that, from the start, physics envy drove economists to think of the social world as a potentially perfect machine. This induced them to model human behaviour to fit the requirements of such a conception. Once economics became formalised in the twentieth century, the requirements of 'ideal' modelling started to dominate theory. Theories needed to be couched in terms of isolated (deterministic) atoms to facilitate modelling. So, the possibility that under conditions X the outcome could be any of a range of outcomes could no longer be allowed. It could be prevented by specifying that in any conditions X there is a unique optimum Y, and that human beings (under the compulsion of 'rationality') everywhere seek and find it. However, in the early phase of the discipline matters were not quite so clear; and the lack of clarity as to whether economists' depictions of human nature were intended to be descriptive or prescriptive has bedevilled the discipline to this day.

The crudeness of its own psychology cuts the economist's picture of the individual off from any serious study of psychology. Until quite recently, economists dismissed the findings of psychology as of no use to them. 'Economics', wrote Lionel Robbins (1898–1984), 'is as little dependent on

the truth of fashionable psychoanalysis as the multiplication table'; he waved away its main rival, behavioural psychology, as 'this queer cult'.[7]

Following the financial crisis, widely attributed to 'irrational exuberance', economists have started to modify their views: behavioural economics is the new vogue. As Andrew Lo says,

> the crisis hardened a split among professional economists. On one side of the divide were the free market economists, who believe that we are all economically rational adults, governed by the law of supply and demand. On the other side were the behavioral economists, who believe that we are all irrational animals, driven by fear and greed like so many other species of mammals.[8]

What is wrong with behavioural economics is that it dubs irrational any behaviour which does not meet the neoclassical specification of rationality. It then tries to formalise that behaviour as rational in the circumstances; for example, it is rational, when faced with partial information, to 'follow the crowd'. These concessions to reality produce incoherence, not progress.

Treating the economy as the sum of individual choices leads to one of economics' greatest defects – its failure to understand the nature of the social world. Economists typically see rational individuals choosing in isolation; as a result they have paid scant attention to the 'sociology of knowledge' – the part played by society in structuring the knowledge on which individuals act. They typically treat social relations as irritating complications to the study of individual choice, rather than as essential components of the choosing process. Interactive behaviour can only be brought into the maximising framework by modelling it as a strategic game, as in the Prisoner's Dilemma, in which actors calculate the value of the payoffs from cheating or cooperating.

Sociology is partly responsible for economists' neglect of it. The demand for sociology as a science of society may have weakened, but there is also a problem with the supply. Contemporary sociologists have, by and large, left the economy to the economists, even though the economists' image of a world in which the 'invisible hand' of the market guarantees social stability is profoundly opposed to the sociological standpoint. Sociology, writes Wolfgang Streeck, must rediscover political economy.[9]

The choice between the individual and the social is not straightforward. One strong defence can be offered for methodological individualism: it guards against treating individuals simply as members of groups, deprived of agency. Its weakness is that it ignores the architecture of choice. Our choices are affected by the social positions we occupy, our place in society's power structure, our reflections on what is good and bad behaviour ('morals'), and our state of knowledge, and these choices in turn help restructure the social world.

In mainstream economics, individual actions typically take place through voluntary exchange in competitive markets, in which, by definition, no transactor has power. This means that its models are blind to the role of power in shaping economic relations: the mythical power of numbers replaces the actual power of elites. The power imbalances between workers and bosses, the influence of money on politics, the role of big business in shaping beliefs and market behaviour – these are all 'outside the model'. The rational agents that economists assume we are would never allow themselves to be bamboozled by advertising. Political science, the science which deals with relations based on power, should be part of the education of every economist, since power structures shape the structure of choice. Karl Marx understood this better than anyone, but his writings are outside the standard curriculum.

History offers students of economics another powerful tool to understand the nature of economic life. All the disciplines have their histories – the histories of how they were done in the past, how they came to be what they are today. Like natural scientists, economists like to claim that the science they do today – the economics of the latest textbooks – is better than the science of a hundred years ago, or even ten years ago. Time, they say, has purged economics of its mistakes.

However, students will discover that economic theory, far from progressing like a giant tapeworm towards better knowledge, is rife with interminable arguments. In the course of this history, no single school has achieved unchallenged dominance. Classical and neoclassical economics may be regarded as the main line of advance, but there are many other schools of thought, including the German Historical School, Marxism, Institutional Economics, Keynesian economics, Behavioural Economics, Ecological Economics, and many others. This pluralism is typical of the

social sciences; but it is rare in the natural sciences. It points to the extreme difficulty of *falsifying* any theory in economics. After centuries of debate, there is still no agreed theory of money. A study of the history of economics is an invitation to join in conversation with some of the greatest dissenters in the field like Karl Marx and John Maynard Keynes. Whatever doubts students may have about the way economics is now done, they will not find themselves alone.

Just as striking as the violent attacks that have been made on mainstream economics is the fact that its methodology has, by and large, remained intact. This is because of economics' undying aspiration to be a hard science. There is an accepted, 'professional', way of doing the subject which exerts a gravitational pull on the way it is done.

Two eminent philosophers of science, Thomas Kuhn (1922–1996) and Imre Lakatos (1922–1974), help explain the roots of methodological persistence. They show that all established sciences erect virtually impregnable methodological defences to safeguard themselves from assault. (For further discussion, see Chapter 10.) These defences include a considerable power of absorbing contradictory thoughts. Economics soaks up heresies, which it turns, where possible, into maths. Occasionally the defences crumble altogether, not so much under the weight of disconfirming facts, as from a changed view of the world. The two candidates for 'paradigm shifts' in economics are the marginalist revolution of the 1870s and the Keynesian revolution of the 1930s. Of these, the marginalist revolution has proved the most *methodologically* durable; its methodological persistence, in fact, doomed the Keynesian attempt to erect an alternative doctrine on neoclassical foundations.

The study of history proper is valuable, because it shows that economic *doctrines*, far from being the universal truths they claim to be, are connected to particular historical conditions and episodes. The conditions of time and place explain not just why they arose when and where they did, but why some doctrines swam while others sank. Influential social theories satisfy 'needs' which arise from outside their own system of thought. Thus the protectionist doctrines of the nineteenth-century German Historical School answered the desire of late-comers to the capitalist feast to 'catch up' successful pioneers like Britain; Marxism tried to explain the miserable conditions of factory workers in the early Industrial Revolution; the

Keynesian revolution offered a theoretical explanation of the persisting unemployment of the interwar years; twentieth-century development economics took up the argument that free trade keeps poor countries permanently poor. Today we have behavioural economics, feminist economics, and other branches. In all cases, doctrines are partly intended to do political work. It is important for students to get a sense of which period and place they are living through, and the power relations of their societies without swallowing the view that economic doctrines are 'merely' reflections of the historical conditions and power structures of the day. If economics fails to give history its due weight as evidence, historians are also guilty of self-absorption: with notable exceptions like Niall Ferguson and Harold James, they have simply failed to engage with economic theory, leaving the field to the econometricians.

Because economics is not a natural science, the 'right' or 'wrong' answer to an economic problem is as much ethical as positive. Economics is the study of people who make ethical judgments: it cannot simply be treated as a matter of good or bad logic or arithmetic. Economists will tell you that moral questions are above their pay grade – 'a matter for politics' – but this is only because they have defined their subject in a way that deliberately excludes them. Yet economists' values determine what they pay attention to, what models they use, and what policies they prefer. Ethics can be used to criticise method.

Except for philosophy (whose job is to sort out everyone else's mistakes) all the disciplines have their biases. Psychologists tend to think of human behaviour as irrational; sociologists, to think of humans as creatures of groups. Historians tend to see only relations of power, and students of politics have traditionally followed their lead. Economics offers a useful corrective to such slanted views. But it also has much to learn from them. A well-known study showed that broadly educated people had better judgment about future economic possibilities than narrow experts.[10] Curiosity may have killed the cat but it leads to better forecasts.

John Maynard Keynes grasped the truth of this when he wrote that:

> The master-economist must possess a rare combination of gifts . . . He must be mathematician, historian, statesman, philosopher – in some degree. He must understand symbols and speak in words. He

> must contemplate the particular in the light of the general, and touch abstract and concrete in the same flight of thought. He must study the present in the light of the past for the purposes of the future. No part of man's nature or institutions must lie entirely outside his regard.[11]

An ideal, no doubt; nonetheless, worthy to be put before the mind of students of economics.

5

MODELS AND LAWS

> When faced with incomprehensible phenomena the human mind gives forth hypotheses, the most plausible, convenient or expedient of which are dressed up into a theory after which tranquillity may be restored . . . this chaos of jarring and discordant appearances brought to order, this tumult of the imagination allayed.
>
> Adam Smith, *Essay on Astronomy*[1]

According to Paul Samuelson it is economics' ability to make quantitative predictions that makes it 'the queen of the social sciences':[2] its theories are engines for generating predictions, which can therefore become the basis of successful policies. The challenge for economics has always been to 'model' economic life in such a way as to generate reliable predictions. The standard technique is to isolate a single motive for action, and deduce its consequences by excluding the influence of other possible motives. This is no different from the technique of other social sciences: for example, political science takes love of power to be overriding. What makes economics 'queen' is that its subject matter is what Marshall called 'measurable motives' – motives whose strength can be measured and compared on the single scale of money. No other social science has found a way of bringing disparate quantities of stuff into such precise relationships with each other. As Lionel Robbins put it: 'Scientific generalisations, if they

pretend to the status of laws, must be capable of being stated exactly.'[3] Furthermore, predictions stated in terms of quantities of money can be properly tested. Hence, economic generalisations are said to be open to improvement in a way that generalisations in other social sciences are not. Economic generalisation can be falsified; generalisations made by other social scientists remain matters of opinion.

How do economists seek to establish their so-called laws? There are two main theories of knowledge in economics (as in all sciences, natural and social): the inductive and the deductive. The empirical theory sees economics as reliant on induction, testing, and refutation. The logical theory portrays economics as a system of logical deduction from axioms – premises 'known to be true'. Provided the axioms are correct, the results will follow. The actual practice of economics is a compromise between these two views. Logical reasoning is at its heart. But its premises are not entirely plucked from the air, and it tries to test the validity of its conclusions against real-world outcomes. There is a third view, to which few economists subscribe, which treats economics as a branch of rhetoric, engaged not in the science of discovering truth, but the art of persuading people of the truth of its own utterances, and by persuasion causing them to behave in a desired way.

Modelling

The answer to the question of how economists seek to establish their laws is through modelling. Modelling is the act of creating a simplified theoretical structure to represent real-world events. In economics, this structure is now overwhelmingly mathematical, with three parts: input variables, a logical process that links them, and an output variable.

Economists claim that building a model is like drawing a map: the object is to leave out cluttering matter, while leaving in place crucial information. A model that is just as complicated as the world is of no use at all, just like a 1:1 map. Economic reality – whatever that is – is too complicated to be directly interrogated; so it must be simplified to the point of caricature. Critics argue that this is simply a rhetorical ploy. The open world is 'modelled' as closed, not to simplify reality, but for mathematical convenience.

The issue is what to include in the map and what to leave out. What one includes in the map depends on what one wants to do. If it is to get from one place to another as quickly as possible the map will highlight coastlines, motorways, express railway connections, and airports. A more leisurely itinerary will require a map with scenic routes. If the modeller wants to map a social terrain, he might populate the map with individuals, and leave out firms and classes, or he might include these. All of this, of course, leaves the modeller, like the map maker, considerable latitude to choose which features of 'reality' to emphasise. There is ample room for ideology. Neoclassical economics claimed it rediscovered the individual buried under the institutional lumber of Marxist theorising.

Models start with hypotheses, which then have to be tested by experiment, or by some other means if experiment is impossible. This is as true of natural science as of economics. Physics has, in nature, its own ready-made laboratory, where events regularly repeat themselves. The social world lacks such stationary features. The standard economic model is typically a theoretical representation of a closed system. But to model an open system as though it were a closed system 'introduces a damaging rift between ontology and epistemology – i.e. between the way the social world actually is, and the way it is represented in economic models. Once in place, the rift cannot be healed.'[4]

Economists use many techniques to 'close' open systems, of which the following are the most important. First is *ceteris paribus* – working out the consequences of a particular change by 'freezing' the other variables specified in the model. David Ricardo's *Essay on Profits* (1815) is an early explicit example of its use: 'We will . . . suppose that no improvements take place in agriculture, and that capital and population advance in the proper proportion . . . that we may know what peculiar effects are to be ascribed to . . . the extension of agriculture to the more remote and less fertile land.' This technique gives you a single starting point leading to a single destination. A second stratagem is to remove potential disturbances from the model entirely by calling them 'shocks' – random events 'exogenous' to the model. A favourite is a technology 'shock'. This preserves the predictive power of the model itself, while allowing for failure of a change in the input variable to produce the predicted change in the output: 'non-

linearity' in maths-speak. A third stratagem, which we have already noticed, is the concept of 'frictions'. This allows for any lags in the adjustment of the different parts of the model to a change in the input variable. It is closely related to the idea of 'transitions' and the short-run/long-run distinction.

Thus the introduction of machinery may make workers redundant in the short run. But it sets in motion forces which will preserve employment in the long run. Given that economists want to achieve a high level of model predictability, these are perfectly legitimate stratagems. But the predictability is too often achieved at the expense of realism – the models are, in effect, rendered immune to criticism. With the increasing use of formal mathematical modelling, the zones of exclusion become ever larger. The subject matter of the enquiry comes to be defined by the requirement of model tractability.

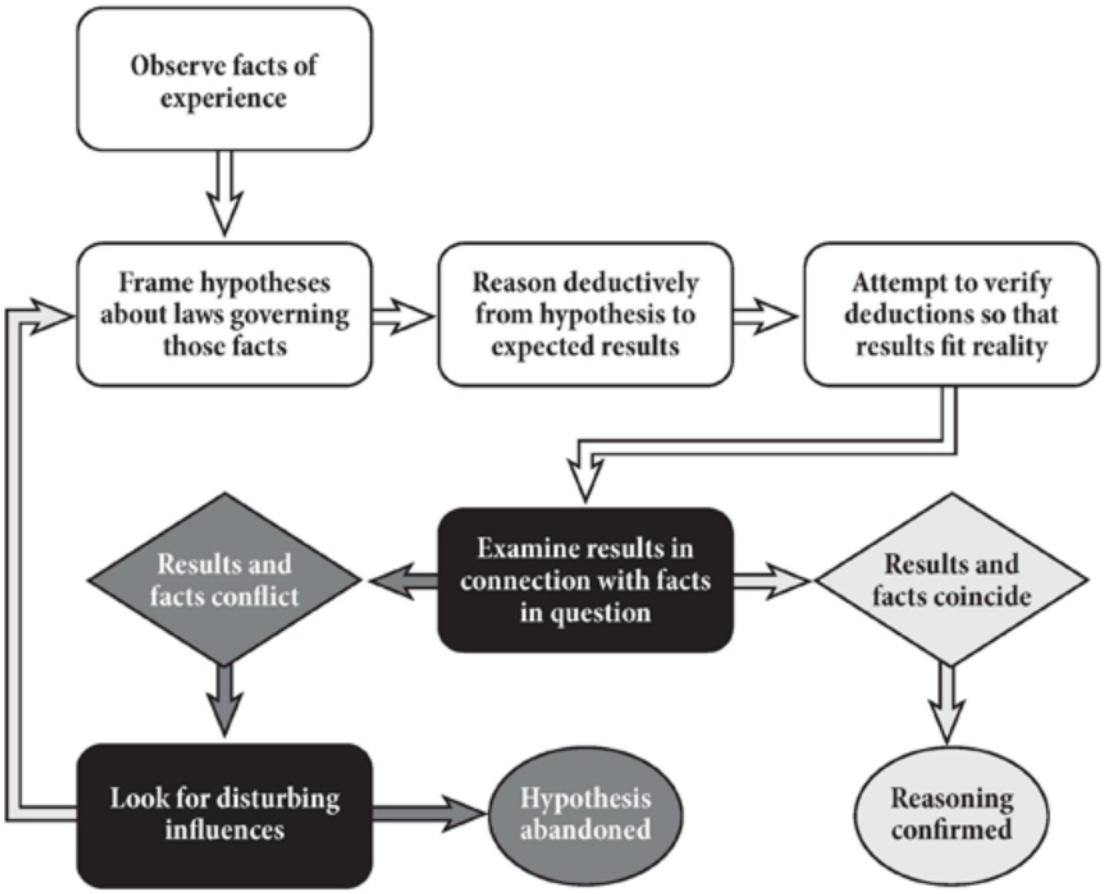
There are three main views of how to construct economic models. The first says you must start with 'realistic' assumptions or your models will be merely fanciful. Second, in his influential paper, *The Methodology of Positive Economics*, Milton Friedman (1912–2006) claimed that the important question is not whether the assumptions of a model are realistic, but whether the model yields good predictions. Any premise will do. If it happens to hit the nail on the head, one can test for whether this was a coincidence or a causal law. The third stresses the deduction of conclusions from self-evident axioms. (Malthus's population theory as described in Chapter 3 is an example.)

The following questions arise. Are models to be thought of as descriptive or prescriptive? Do models aim to show how people behave or get them to behave as the modeller thinks they ought to behave? The normative or prescriptive purpose of modelling is hardly ever acknowledged, because economics is supposed to be 'scientific' and 'value-free'.

The economist Jevons put one view of the task of economics simply: 'The investigator begins with the facts and ends with them.' In his conception, there are three stages in model-building: the inductive hypothesis, the deduction of a conclusion, the testing of the conclusion against reality.[5]

The process may be illustrated as follows. An observation suggests a ‘conjecture’ or ‘hypothesis’ as to why something may be the case. You then develop a theory which involves establishing a causal link between your conjecture and other factors called variables. The deductive stage involves working out the logical consequences of your hypothesis. You then test the conclusion against reality. Jevons realised that a deductive argument can do no more than link a set of premises to a set of conclusions. If the assumptions are unrealistic, the conclusions (predictions of the model) will not hold in the real world. So in his view the assumptions need to be realistic.

A standard workhouse model in modern macroeconomics is the Phillips Curve. The statistician A.W. Phillips (1914–1975) noted (1958) an empirical relationship (‘correlation’) stretching from 1861 to 1957 between inflation and unemployment.[6] This suggested that governments could ‘trade off’ a bit more inflation for a bit less unemployment, and vice versa.



4. The Method of Modelling.

The problem with the original Phillips Curve was the disappearance of the postulated trade-off between inflation and unemployment in the later 1960s. To explain this 'change in the facts' a hypothesis was suggested: rational agents 'learn from experience'. They come to realise that the current inflation rate is the rate they can expect and adjust their wage-bargaining behaviour accordingly. This resulted in the 'expectations augmented' Phillips Curve, which predicts that over time, government attempts to reduce unemployment by allowing a bit more inflation lead only to accelerating inflation. Notice that in this model there is no attempt to investigate changes in institutional facts (in trade union organisation, historic levels of unemployment, among others) which might explain the breakdown of the original Phillips Curve: the single postulate of 'utility maximising behaviour' does all the work needed.

A close inspection of this procedure points to some of the difficulties inherent in model construction.

1. What is the status of the 'facts of experience'? Are they based on casual observation, observed regularities, interpretations of the facts, or known *a priori?* In other words, are they already 'contaminated' by prior conceptualisations? – for example, that human behaviour is rationally calculated?

2. What lies behind the inclusion of some, and the exclusion of other, possible causal variables? What, in other words, guides the modeller's judgments of relevance?

3. What constitutes verification? Results are rarely black and white, so how dark a grey can we accept? How large a pile of 'disturbing influences' is it possible to accumulate before the theory is more exception than rule, and should be abandoned? What if the results and facts appear to coincide, but do so only by chance?

The facts of the matter

In practice, economists almost never start with the facts; there are too many. Nor do they normally start with 'vigilant observation': numbers arranged as

statistical series from which they try to discern patterns and suggestive anomalies. They start with a hypothesis and then try to prove it. The hypothesis is not 'plucked from the air'. Nor is it based on systematic observation, even though economists often appeal to the 'indisputable facts of experience'. Rather, it is based on the claim to 'direct acquaintance' or 'intuitive' knowledge of how humans think. Ronald Coase (1910–2013) recalled the English economist Ely Devons (1913–1967) saying to him, 'If economists wished to study the horse, they wouldn't go and look at horses. They'd sit in their studies and say to themselves, "What would I do if I were a horse?" And they would soon discover that they would maximise their utilities.'[7] This joke gives a profound insight into the economic method. Economists see themselves as forming their theories by looking into the minds of their subjects and seeing how they think. This is what enables them to make sharp predictions about their behaviour. 'Vicarious problem solving,' writes Nobel Laureate Thomas Schelling (1921–2016), 'underlies most microeconomics.'[8]

So economists' models may be interpreted as starting with intuitions about what goes on in the horse's mind.[9] They claim they are merely formalising 'models' which are already 'there'. But this may not give you a good way of understanding behaviour. The chances are they have put into the mind of the human horse what they want to find there. So a key question concerns the relationship between the economists' hypotheses of human behaviour and how humans actually behave. Are economists' models intended as replications or simplifications of actual behaviour, or are they intended to create behaviour consistent with the economists' models – to create self-fulfilling prophecies, so to speak? It seems pretty obvious that economic models are intended to be both descriptive and prescriptive, wobbling between claims that this is how humans behave in fact, and this is how they should behave, both converging on a predictive claim.

Paul Krugman (b.1953) has described the model-building process as follows: 'You make a set of clearly untrue simplifications to get the system down to something you can handle; those simplifications are dictated partly by guesses about what is important, partly by the modelling techniques available. And the end result, if the model is a good one, is an improved

insight into why the vastly more complex real system behaves the way it does.'[10] The argument here is that economists need the untrue simplifications to get the generalising machinery going. But it can be argued that heroic (untrue) assumptions should have no place in a discipline intended to be useful. To start off one's reasoning with a basic premise (axiom) that is immune to challenge cannot justify certain knowledge of a conclusion, unless one (irrationally) accepts the premise as true.[11]

Macroeconomic models have tried to get beyond 'untrue simplification'. The economist Nicholas Kaldor wrote,

> The theorist, in my view, should be free to start with a 'stylized' view of the facts – i.e. concentrate on broad tendencies, ignoring individual detail, and proceed on the 'as if' method, i.e. construct a hypothesis that could account for these 'stylized' facts, without necessarily committing himself on the historical accuracy, or sufficiency, of the facts or tendencies thus summarized.[12]

A good hypothesis accounts for the stylised facts. Kaldor's was a notable attempt to ground macroeconomic models in 'vigilant observation' rather than 'inner understanding' of human nature. However, an over-enthusiastic reliance on stylised facts may lead the modeller seriously astray when the facts change.

All economic models have a tight logic, amounting to mathematical proof of the conclusion. The name of the game today, as depicted by Nobel Laureate Robert Lucas (b.1937), is to 'get logically consistent mathematical conjectures of various degrees of complexity'. But economics cannot live by logic alone. To be useful a logical argument has to be based on true beliefs about something. Logic can tell you nothing about the real world; it can only tell you about itself. Students should be aware of the pitfalls of reasoning *a priori*: the argument 'If all swans are white, and X is a swan; therefore X is white' is valid in logic, but not in fact since not all swans are white. If the starting point was that 'most swans are white', then one will know more about what colour swans actually are but won't be able to make a definite prediction about the colour of the next one encountered.[13]

The most important name in the philosophy of testing is the Austrian philosopher Karl Popper (1902–1994). Popper believed that what

demarcated science from non-science was not whether theories could be proved but whether they could be falsified. Popper's point was not that verification is less powerful than falsification, but that it is impossible. Scientific laws claim to hold true universally, and to verify a universal statement is impossible for finite minds.

However, falsification is also rarely possible. Even in the natural sciences there can be no conclusive disproof of a theory in the strict logical sense that Popper wants because it is difficult to know which of several hypotheses you are falsifying.[14] It is always possible to say that the experimental results are not reliable, or that the discrepancy between observation and fact will disappear with the advance of understanding, rather like Cesare Cremoni's doubts about whether Galileo's telescope had been tampered with.[15] Although a lot of scientists still swear by Popper, among philosophers of science his view has long been rejected. The problem, as Lakatos pointed out, is that scientists don't reject theories the moment they encounter problems; they construct 'auxiliary hypotheses' to account for the disconfirming instance.

Popper believed that his verification principle applied equally to the natural and social sciences: in fact, he failed to distinguish between the two. But falsification in economics encounters even worse problems than in any natural science, because the ubiquity of the 'other things staying equal' condition serves to immunise an economic theory against the disturbing influence of untoward events. One can get robust predictions only by waving away the disturbing causes.

Testing hypotheses in economics encounters the general problem of testing faced by all social sciences. First, although one can, with some difficulty, do experimental work on a small scale, it is impossible to experiment with whole economies; the second is the weakness of econometrics, the substitute for experiment.

Economists are mostly debarred from using the experimental method, typical of applied natural sciences like medicine, to test their hypotheses. Suppose you invented a new drug which you expect to lower cholesterol. How would you test for it? In a laboratory test you could secure the equivalent of the 'vacuum' situation by ensuring that the two sets of lab rats are subjected to identical conditions, except that only one group is given the

drug. If the outcome between the two groups is identical this would amount to a refutation of the hypothesis, calling for a new one. A difference of outcome would corroborate the hypothesis that the drug lowers cholesterol. But it would not confirm that it does so in all, or even most, conditions, because these have been equalised by design. So no irrefutable 'law' has been established, but perhaps a useful indication, which can be further refined.

The technique of randomised control trials, borrowed from medicine, suggests a way round the difficulty of conducting controlled experiments with rats. In the lab experiment you have taken steps to ensure identical initial conditions. But you might achieve the same result by administering tests to individuals selected at random – that is, those whom you have no reason for thinking are different in any relevant respect. The trial then proceeds in the same way. Divide your test subjects into two groups, at random, then administer a 'treatment' to only one of the groups, and compare the results.

This method was used to evaluate the famous PROGRESA scheme in Mexico, which involved providing cash transfers to households for sending children to school. The finding was that more education resulted in higher wages. It is unlikely that a trial of this kind would satisfy a convinced Popperian, but it is fit enough for purpose.

The randomised evaluation of public policy interventions works well in fields like public health economics, where one can plausibly assume equal susceptibility to disease and interventions. It has been used to develop effective vaccines for treating pneumonia and meningitis in developing countries.[16] But it is useless for testing the effect of interventions in 'open' systems, where the constancy of the underlying structures cannot be plausibly assumed. Each country has its own particularities of geography, climate, culture and institutions, so would make poor experimental controls. Even if this were not the case, the sample size would be too small to draw the types of robust conclusion required.

Econometrics

By far the most prominent testing technique in economics is econometrics. The economist Guy Routh described it as 'mock empiricism, with statistics

subjected to econometric torture until they admit to effects of which they are innocent'.[17] Econometrics is a kind of statistics, but one in which empirical evidence enters not as a foundation for the argument, but as a health-check on the conclusion. It is used not to display the facts of the world in statistical form but to test the statistical significance of the relationships hypothesised by the model. We run a regression to estimate the quantitative influence of the independent variables on the dependent variable, according to a model specification set out by the researcher. Typically, this amounts to assuming a linear (straight line) relationship between the independent variables and the dependent variable (or some transformation of it).

Two problems are commonly raised with econometrics. Firstly, it is almost impossible to isolate the hypothesis which needs to be tested from the many other hypotheses which have had to be assumed in order to make the test possible. This includes the possibility that there might be a circular relationship, where the variable you have assumed is purely dependent exerts influence on the independent one, or that important aspects of the relationship are omitted from the model. This objection highlights the fact that a correlation (the association in time of two events) tells you nothing about the causal relationship between them. A celebrated example of an econometric 'proof' which has failed to escape from the trap of circularity is the claim by Alberto Alesina (b.1957) that cutting government spending in a slump causes economic recovery.[18]

Secondly, time-series cannot establish the laws which economists seek. If the time-series is too short, there is not enough data. If it is long enough, the conditions are not stationary. So something true at one time may not be true at another. The heterodox economists are right. All so-called economic laws are dependent on time and place.

There can also be too few observations. Studies by Harvard University's George J. Borjas and others suggest that net immigration lowers the wages of competing domestic labour. Borjas's most famous study shows the depressive impact of 'Marielitos' – Cubans who emigrated *en masse* to Miami in 1980 – on domestic working-class wages. In reply, others pointed out that there were sampling issues: the census bureau had recently made an effort to sample more black males, who tended to have low incomes, and

the sample was too small not to be swayed by this. Borjas in turn accused his critics of bad faith.[19] Far from clarifying the matter, econometrics had spun everyone around in circles. There are too many examples of studies whose econometrics were subsequently discredited, either by spreadsheet mistakes, or cognitive bias.

These problems point to the fundamental weakness of econometric testing: that the conditions needed for its success arise only in controlled experimental situations. Most econometricians recognise that these conditions fail to hold strictly but proceed as if this wasn't important. They fail to understand that the very act of writing papers in learned journals using these techniques gives authority to faulty procedure. Students are told: if everyone does it this way, it must be right. Economists' health warnings are like the small print in a statement of business accounts which no one reads.

Modelling complexity

Following the crash of 2007–2008, there has been a surge of interest in how best to model 'complex' systems. This stemmed from the realisation that the simpler models like the 'efficient market hypothesis' completely failed either to foresee or understand the crash. 'Complexity refers to the density of structural linkages and interactions between the parts of an interdependent system'.[20] In other words, because there are so many relationships and potential feedback loops between variables, even small changes have the potential to produce large knock-on effects. This not only makes it difficult to understand the system intuitively, but also excludes traditional modelling techniques which generally require sparse structural linkages. The chief approaches to understanding complexity are agent-based modelling, network analysis, and system dynamics.

Agent-based modelling tries to avoid fallacies of composition that would occur by using the 'representative agent' hypothesis, which assumes that the entire economy can be represented by a single individual who thinks like everyone else. Instead, it simulates the actions and interactions of a multitude of agents who may have different characteristics and display adaptive behaviour. The modeller sets up relationships between the agents and defines the conditions of their world. The fictional agents are then left

to interact, possibly under a shock or change in conditions of some kind. The simulated outcomes churned out thus constitute the results of the model. These outcomes can serve as indicators of what will happen in the real world without the need for further interrogation.

Network analysis studies economic networks, which are ‘webs’ whose nodes represent economic agents (individuals, firms, consumers, organisations, industries, countries, etc.) and whose links depict market interactions. This is useful for studying the rise of networks in the global supply chain. The most important networks today are programmed computer networks.

System dynamics, derived from Forrester’s (1971) attempts to model the world ecosystem, take a similar approach but focus on links between aggregate variables rather than agents. These can be economic variables such as GNP or capital stock, but could also refer to physical quantities such as forested areas or oil stocks, which has made this technique particularly popular in ecological economics.

Although an improvement on mainstream methods, these techniques presuppose the same atomistic ontology in order to generate their predictions. They must in turn make assumptions about behaviour and relationships. These may be based upon observation, intuition, or simply plucked from the air, but must necessarily be simplified or idealised descriptions of the real world. They will be internally and logically consistent, but the results largely follow from the premises: they are not really ‘new knowledge’, and in any case the ‘art’ of calibrating the model is often what really generates the results. The chaos of the interacting agents and conditions can throw up vastly different results even from the same initial conditions, so the best a simulation can do is act as a useful guide to the range of possible outcomes, and shed light on the dynamics of the system.

It might be tempting to apply the well-known aphorism ‘garbage in, garbage out’ to economic modelling. Certainly there are cases where this is true, but it does not apply universally. The purpose of the modelling exercise is key: if precise predictions of real-world outcomes are desired, models are likely to disappoint, except in special situations. If they are intended as tools to investigate the consequences of certain assumptions,

clarify thinking, and make general claims about how events might respond to certain actions, they are useful.

Platonic modelling

Economists may construct models as ideals, just as a model in ordinary language can mean not a simplification (as in a model aeroplane) but an ideal of goodness or beauty: perfect 'forms', of which objects in the everyday world are imperfect copies. Platonic models are pictures of what reality might be like if it attained to an ideal state. One can think of them as 'benchmarks'. To the economist this means a state of perfect efficiency: the efficiency of a perfectly frictionless machine. They have a powerful ally in computer technology, able to assemble and process masses of data in 'real time'. This promises to realise, at no distant date, the economist's vision of the human as a perfect calculating machine.

The writings both of neoclassical economists and technological utopians reveal the prescriptive nature of their callings. They are allies in their ambition to 'make the crooked timber of humanity straight'. So economists' theories are meant to inspire greater efficiency. There is some evidence that the prescription works. In a marvellous book, *I Spend Therefore I Am*, Philip Roscoe (2014) reports studies which show that students of economics were markedly more calculating than those of other subjects, though whether it was their calculating nature which drew them to economics, or economics which made them more calculating, is not clear. 'Rational expectations' models are examples of such ideal modelling. They assume that economic agents are perfectly rational and perfect processors of their information. The assumption hides the hope that in time people will come to behave in the way the ideal model says they should.

Science versus rhetoric

Deirdre McCloskey is the best-known exponent of the view of economics as rhetoric. Coming from a mainstream economics background, she denies that economics can prove its arguments, because there is no possibility of falsification. There are no true or false arguments, only persuasive and unpersuasive ones. Maths is neoclassical economics' most emphatic

metaphor: the economic researcher has only to produce a correlation, and the statistically unsophisticated are persuaded he has discovered a cause. Nevertheless, McCloskey believes that the rhetorical character of neoclassical economics is socially useful, because it strengthens the case for free markets.[21]

To say that economics is purely rhetorical is to deny that there is a reality outside the language of persuasion itself. How does rhetoric work? It normally starts with an appeal to some thought or prejudice already in the mind of the audience, like 'we all know that . . .' The rhetorical articulation of this 'common sense' makes it *consciously* common. This, as we have seen, is precisely the way all economic arguments start, with the 'facts of experience' being the 'premises' of the deductive logic. The rhetorical character of this procedure is disguised by the claim that what 'we all know' is true.

Economics has to assert the truth of its premises to generate its prized 'quantitative predictions'. But this is a rhetorical device. The 'facts of experience' cannot provide the universal premises necessary to demonstrate the truth of the conclusion. There are too many contrary facts. This does not make the conclusion utterly false. It makes the argument incomplete. Rhetoric is the art of incomplete argument, a 'heuristic' device, or story, to point the mind in the right direction. In this sense all the social sciences are rhetorical. This simply means that the conditions required to make them universally true do not hold, or only hold under special conditions. They are only partially true.

The claim that economics is rhetoric has been heavily influenced by post-modernism, the movement which has dominated cultural studies since the 1980s, which claims that all arguments in the humanities are of the persuasive rather than demonstrative kind. As Jacques Derrida (1930–2004) put it, 'there is no outside text': there is no reality outside the circle of language. Post-modernist literary criticism 'deconstructs' the 'text' by shifting attention from the truth of what is being asserted to the means by which people are persuaded of its truth. From this perspective, economic modelling is a persuasive undertaking: it does not aim to discover truth, it tries to persuade people of the truth of its own 'text'. All reality is 'socially constructed'.

Philip Mirowski carries the argument further by saying that natural sciences, too, are built on persuasive utterance. There is a fundamental gap between our thought and reality which can only be bridged by metaphor, simile, analogy. Logical proofs are part of the persuasive machinery.[22]

There are three valuable implications of this approach. First, it emphasises that stories or narratives are the ways in which people try to make sense of complex situations. They assume, that is, that much social landscape is mysterious, or uncertain. Their ways of making sense of it should not, therefore, be considered irrational, but rather reasonable in the circumstances. Second, it points out that belief in the story rests on confidence in the story-teller. This is undoubtedly true: knowing that our own predictions are worthless, we rely on the testimony of those supposedly better informed. Third, while the stories are not the engines of prediction envisaged by Samuelson, they illuminate problems which escape formal modelling. The question, then, is whether economic modelling can improve significantly on story-telling or whether it is part of the story-telling.

McCloskey is almost unique among methodological critics of mainstream economics in viewing the overall programme of the mainstream as a success. Economics may be rhetoric dressed up as science, but its effects are positive. Quite simply, it tells the right story. Unlike most of those who think of economics as rhetoric, McCloskey believes that the market system has ensured progress and prosperity. The scientific pretensions thus take on a life of their own; they are not methodological mistakes, but choices of communication strategy which allow economics to be seen to be consistent with the dominant scientific-rational mode of engagement with the world.

However, the claim that economics is just rhetoric is itself rhetorical, because it fails to distinguish between what makes some arguments persuasive and others unpersuasive. Economists may tell stories, but these are stories about something. They may be reflections of folk stories, but where do these stories come from? The stories we tell each other may not be the complete truth, but an incomplete argument is not the same as one that is just made up. It has to have some basis in experience and evidence. Without it, it would not be persuasive. The point to remember is that

economics is not the only ‘text’ in the social sciences. There are many ‘truths’ out there about the human condition, of which economics is just one.

So is economics a science?

Economics is not like a natural science in that it does not, and cannot, use experimental methods to generate laws. A scientific theory cannot require the facts to conform to its assumptions, but this is what economics tries to do. The failures of mainstream economic theory are not, on the whole, due to the internal inconsistencies of its models, but the failure of the models to account for observed facts. Except in special cases, economics has not advanced beyond what Rosenberg calls ‘generic’, that is, qualitative, predictions: predictions of broad tendencies, not of specific events.[23]

Macroeconomic models have fared particularly badly. The big Keynesian macro forecasting models broke down in the 1970s, because the assumed stable relationships between aggregates, like the consumption function or the relationship between unemployment and inflation, broke down. Models which start with large ‘stylised facts’ have fallen victim to breaks in trend. For example, Kaldor’s ‘law’ of a constant wage share in national income fell foul of globalisation. Verdoorn’s ‘law’ of increasing returns to scale in manufacturing industry became much less relevant when manufacturing ceased to be a major part of production in advanced economies. The Kuznets Curve, which predicted decreasing inequality after a period of growth, has broken down, partly because the state became indifferent to questions of income distribution. Such breaks in trend – partly at least – reflect changes in behaviour caused by the discovery of the trend, and the attempt to exploit it for policy purposes.

It is tempting then to abandon the attempt to map the movement of macroeconomic variables directly, and concentrate on mapping the supposedly unvarying (maximising) motives of individual agents. This, indeed, was the response of the mainstream to the failure of the Keynesian macroeconomic forecasting models. Micro-models, it was claimed, would be better forecasters than macro-models. But this hinged on economists getting human behaviour right. The failure of the neoclassical financial models to predict not just the crash of 2008, but even its possibility,

suggests that their account of human psychology was deeply flawed. It was not just that they got the ‘facts’ of human behaviour wrong; but that, from the rhetorical point of view, they put much too much faith in the persuasive power of economic theory to make behaviour conform to the assumptions of the model.

The conclusion to which we are drawn is that there are no ‘laws of economics’ valid at all times and places. At best, theories can lead to approximately reliable predictions over such time periods as other things stay the same. This is true of short periods in particular markets and in specialised areas such as in health economics. Macroeconomic forecasts are reliable over very short periods but not when the parameters are shifting.

One important implication of this is that mathematics plays an oversized role in modern economics. The role of maths in any social science is to formalise its logic, and to make specific the relationships between different variables. But the wholesale formalisation of economics rests entirely on the premise that the variables of interest can readily be expressed as mathematical quantities. Many behavioural facts such as friendship or love of power do not lend themselves to such treatment. The tight logical relations, therefore, simply exhibit the theoreticians’ prowess in tight logical reasoning.

As Robert Solow (b.1924) has pointed out, ‘there is enough for us to do without pretending to a degree of completeness and precision which we cannot deliver’. The functions of analytic economics are ‘to organise incomplete knowledge, see connections that the untrained eye might miss, tell plausible causal stories with the help of a few basic principles, make rough quantitative judgments about consequences of economic policy and other events. These are worth doing, science or not.’[24]

It’s because economics is not a science that it needs other fields of study, notably, psychology, sociology, politics, ethics, history to supply the gaps in its method of understanding reality. We should not be afraid to say to the economist, ‘There are more things in heaven and earth, Horatio, than are dreamt of in your philosophy’. The task is no less than to reclaim economics for the humanities.

13

RETREAT FROM OMNISCIENCE

> In the greater part of our concernment, God has afforded only the Twilight . . . of Probability, suitable, I presume, to the state of Mediocrity and Probationership He has been pleased to place us in here.
>
> John Locke, *An Essay Concerning Human Understanding*

Mainstream economics gets human behaviour wrong in two ways. It endows humans with excessive *power* to calculate; and ascribes to them an excessive *desire* to calculate. It ignores, that is, uncertainty and people's attachment to each other. These failures are rooted in a method of analysis whose major premise is individual maximisation. As Keynes well put it, the error of economics lies not in its logical inconsistency, but in the 'lack of . . . generality in its premises'.[1] There is a large gap between the account economics gives of human behaviour and behaviour as it is actually exhibited. This gap it hopes to close not by broadening its own premises, but by narrowing what it means to be human to the simple point of calculation, and empowering calculation by big data and accelerated computing power. The result is a growing disjunction between what economists think and what many people feel, which expresses itself in an explosion of social discontent. Mainstream economists have not looked deeply enough into the 'mind of the horse'.

In what follows I will try to draw together the book's two main threads of argument, those concerned with the epistemology of economics, and those concerned with its ontology.

Epistemology: risk and uncertainty

The first issue is about how much we do, or can, know about the future. Economics looks into people's minds and discovers utility maximisation. This then becomes the basis of its own theorising. A much more modest, and accurate, claim would be that people do the best they can under the circumstances. These circumstances include uncertainty.

We owe the distinction between risk and uncertainty to both Frank Knight (1885–1972) and John Maynard Keynes. 'Risk' applies to situations when the chance of a possible event is quantifiable; 'uncertainty' implies a lack of any quantifiable knowledge of the chance. (Equivalently, risk refers to all outcomes that can be insured against, uncertainty to those which cannot.) Mainstream economists do not recognise this distinction. They believe that individuals can accurately calculate the odds of any action turning out one way or the other. This is because they treat the economy as a closed system, like a game of draughts. The financial system is explicitly theorised this way by Chicago economists: the risks of all assets are said to be 'correctly priced on average'. The collapse of 2007– 2008 was therefore impossible. Even economists who reject the full rigour of the Chicago school are professionally constrained to use the language of risk whenever they talk about forward-looking choices. People have 'risk profiles'; interest rates measure 'appetite for risk'; government bonds are 'risk-free' (except if they are Greek!), asset prices measure risk aversion and rational expectation and so on. Yet turn to the financial press, and we learn that the one thing businesses can't stand is 'uncertainty', that they are always calling on governments to 'end uncertainty' about this or that. Inflation-targeting was devised to 'end uncertainty' about the future course of prices. What on earth is going on?

The reason why 'Knightian uncertainty' has proved more acceptable to the profession than 'Keynesian uncertainty' is that Knight confined it to 'disequilibrium' situations, whereas for Keynes uncertainty determines the nature of the equilibrium itself. In his book *Risk, Uncertainty and Profit*

(1921), Knight explains profit as a reward for entrepreneurship, or innovating a new product, and by definition there can be no probabilities attached to the success or failure of an innovation, because an innovation is a new event. So profit is a reward for a successful venture into the unknown. Such rewards of enterprise are to be distinguished from the 'normal' returns to capital; profit is a temporary monopoly phenomenon which will be competed away as the innovation is generally adopted. Economists are just about prepared to admit uncertainty on those terms. For Keynes, uncertainty contaminates the investment demand schedule as a whole, and not just enterprise. There is no 'normal' rate of return: there is simply an expected rate of return, governed by uncertainty.

There are two further reasons for the failure of Keynesian uncertainty to grip the mainstream. First, Keynes himself called his discussion of uncertainty in Chapter 13 of the *General Theory* a 'digression', and standard interpretations of the theory take him at his word. Second, his fragmentary account failed to distinguish clearly between those parts of an economic system which could be considered risky and those which were inescapably uncertain. This is why post-Keynesian attempts, like those of George Shackle (1903–1992), Hyman Minsky (1919–1996), and Paul Davidson (b.1930), to ground economics in epistemological uncertainty have made so little headway.

However, Keynes bequeathed another 'general theory', which does deserve serious consideration as a foundation for a reformed economics. This is his theory of probability, offered in his *Treatise on Probability*, a neglected masterpiece conceived before Keynes thought of himself as an economist, in which he expounds what Rod O'Donnell calls 'a general theory of rational belief and action'.[2] It was not published until 1921, the same year as Knight's *Risk, Uncertainty and Profit*, but the germ of the idea dates back to 1904, when Keynes was a student at Cambridge University.

Keynes, too, looked into the 'mind of the horse', but he didn't see maximisation, rather an attempt to behave reasonably under different degrees of certainty. His key move was to distinguish rational belief (or expectation) from true belief. Standard rational expectation theory identifies the two, because to have a rational expectation of an event is to have accurate knowledge of its probability. Keynes claimed it was rational to

believe that something would probably happen on the basis of the evidence supporting it, but that the evidence might be too sparse to deliver a numerical probability that it would happen.

Keynes recognised three classes of probability in descending order of certainty: a small class of cardinal probabilities, a much larger class of ordinal probabilities, and a third class to which no probability can be attached.

Cardinal probabilities are ratios, expressed as fractions. They are either known *a priori* (mathematically) or as a result of likeness to previous events. For example, if one smoker out of ten has died of lung cancer, the probability of smokers dying from cancer is 10 per cent. This second set of numerical probabilities is the standard domain of risk as recognised by actuaries: for example, all fire insurance premia are based on the number of houses which have burnt down in a district over a period of time relative to the total number of houses in it. At the opposite extreme is uncertainty, as both Keynes and Knight define it, but which the mainstream denies: a situation where we have no scientific basis for calculating a ratio. However, in between lie Keynes's 'orders of magnitude' which are orders of likelihood – 'more or less likely' – not exact ratios: we may say that one probability is greater than another, without knowing how much greater. He sums up as follows: 'The magnitudes of some pairs of probabilities we shall be able to compare numerically, others in respect of more and less only, and others not at all.' Keynes believed that it is in this middle ground of ordinal ranking that most of our rational choices have to be made.[3]

In the neoclassical epistemology, by contrast, all probabilities have numbers. They start off as odds you would give on, say, a horse winning a race. This requires no knowledge of past performance of the horse: rationality requires only that your bets should be internally consistent, such that nobody can construct a 'Dutch book' against you.[4] Subjective beliefs are transformed into objective probabilities by applying Bayes' theorem, a rule for updating subjective probabilities in the face of evidence.[5] If one assumes, as hardline rational expectation theorists do, that agents are fully equipped with up-to-date knowledge of the likelihood of any future event, then they are in a position accurately to price risks.

Keynes's 'general theory' of rationality is a big improvement on the neoclassical theory. It avoids the trap of calling behaviour 'irrational' where it does not conform to the neoclassical standard of rationality. It offers a way of distinguishing between closed, partly closed, and open systems. It challenges economics to think about human behaviour under varying conditions of knowledge, and not take the easy mathematical route to prediction. In doing so, it points the way to a unified social science methodology.

Ontology: what exists

The project of improving how to do economics cannot rely on a return to Keynes. Keynes's chief failing is an underdeveloped ontology – one which lacks a genuine sociological or historical perspective. He recognises that 'the atomic hypothesis which has worked so splendidly in physics breaks down in psychics', and gives examples like the 'fallacy of composition' and the 'paradox of thrift'. But he leaves it there.[6]

So an improved ontology – the study of what exists and of the basic constitution and nature of social phenomena – should be the second pillar of a reformed economics. The orthodox map of reality is peopled only with individuals; to the extent that they are recognised at all, groups and institutions exist only as instruments, tools like technology. This 'methodological individualist' approach cuts economics off from understanding a large part of human behaviour, as a consequence of which it often gives faulty advice. It fails to understand the hold of religious national and group loyalties, attachments, identities – all that Weber calls 'communal' associations – and the extent to which these modify its picture of the maximising individual; it fails to understand the power of self-understanding and the way social positions shape self-understanding; it fails to understand the role of ideas, power, technology in shaping choices, including its own; it fails to understand the historical contingency of some of its universal doctrines; and it is indifferent to its own history.

A more accurate map of social reality would feature at least three entities with 'agency': individuals, governments, and 'corporations', linked together through an intricate network of relationships. The meaning of the first two is clear enough: by 'corporations' I mean all those groups

intermediate between the individual and the state which provide valued services to individuals, and to whom individuals relate: local governments, churches, universities, voluntary associations, firms, trade unions, banking systems, digital systems, social movements, and many others. A structure in which public goods (and bads) are provided by private bodies for reasons of prestige or duty or profit – as has been the case throughout history – cannot be fitted into a binary system of state and markets. One might think of the economy as a 'mesoeconomic' system, with the state administration at the top, the individual at the bottom, and a variety of intermediate institutions in between; the whole complex contributing to economic outputs. In the international system, the national state is itself an intermediate institution between the individual and supranational organisations.

The importance of structures is that they affect individual motives and thus shape individual behaviour. It's not behaviour *of* groups, but behaviour *in* groups which we should try to understand. Behaviour in groups cannot be understood as the outcome of individual calculations of self-interest, however hard the New Institutionalists try. Love, fear, courage, loyalty, greed, treachery, worship, and many other traits humans regularly display and admire or condemn can only be understood in a group context.

Proper understanding of both the roots and the logic of collective action leads us far from the neoclassical path. Cooperation did not start with the realisation that it could reduce transaction costs. Economists might say that this is just a precise way of talking about the costs of individual action. And there are such reasons for cooperation. But these do not lead to any deep understanding of sociability.

The weakness of the neoclassical perception is seen in the standard account of the origins of trade. In Paul Samuelson's words: 'A great debt of gratitude is owed to the first two ape-men who suddenly perceived that each could be made better off by giving up some of one good in exchange for some of another.'[7] Most economists have favoured the bartering savage story because it leaves out society. The point is, though, that in order to enter into such transactions you have to be a social animal to start with, as Durkheim pointed out, though indeed a uniquely inventive one. Individuals don't voluntarily *choose* to be social; they are destined to be both social and socially inventive. Relative social instability is thereby built into the human

condition. That is why it is impossible to freeze the frame, except temporarily and locally.

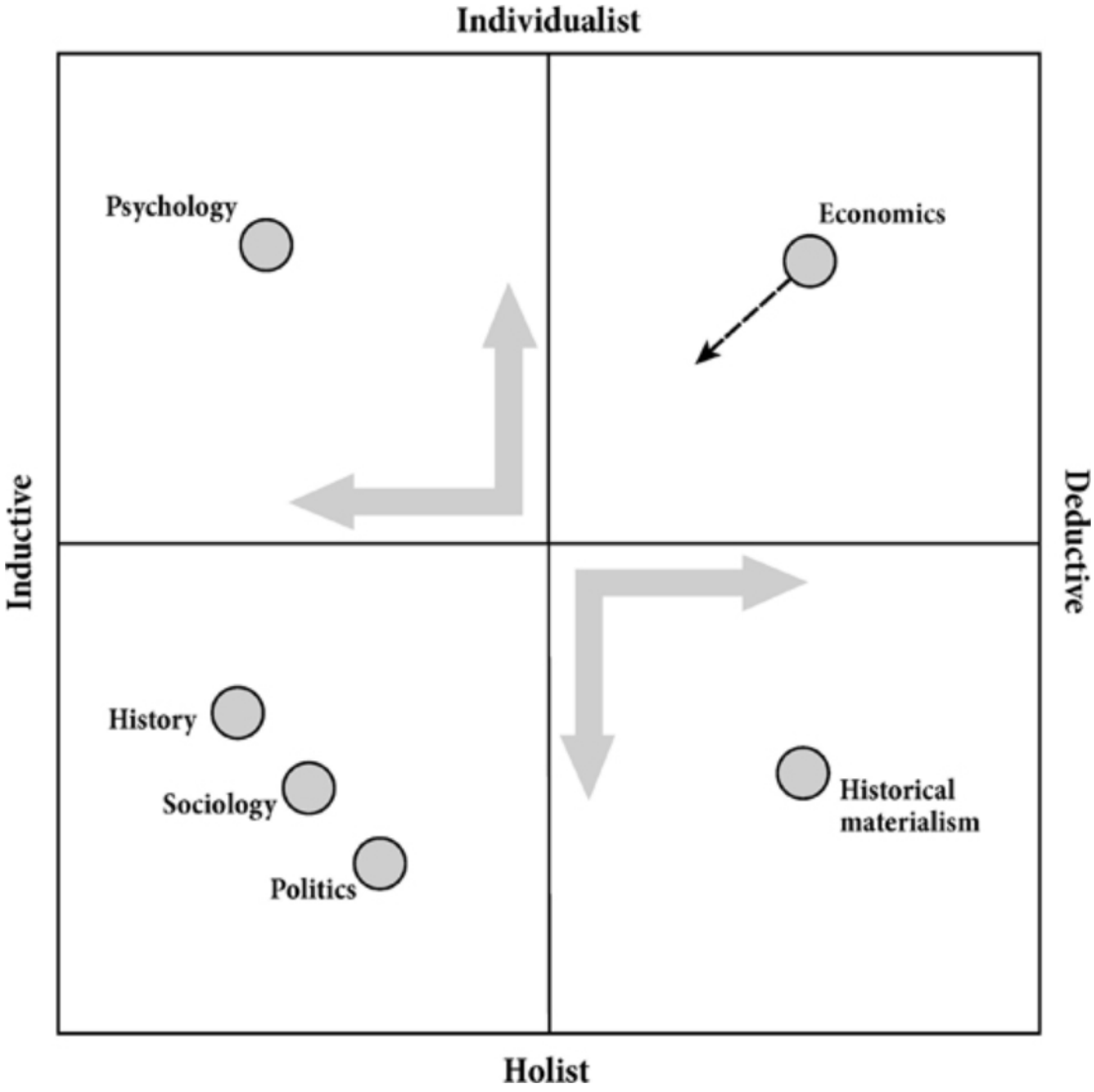
We are left with a conundrum which is hard to resolve. When economists 'look into the mind of a horse' do they really see what is there, or only the sermons they have already planted in it? In other words, is economics descriptive or prescriptive? This book suggests that it is intended to be both. Insofar as it is descriptive it is plainly inadequate; but is it not possible that description may, over time, come to resemble prescription? That people may actually behave more and more as economists tell them they do behave? This would be an ironic inversion of Bayes' theorem, with the objective reality coming increasingly to resemble the subjective bets economists place on humankind. To transform human nature, not just to describe it, has always been the dream of social engineers, as today it is that of the techno-utopians. It is the foundation of the doctrine of progress. But how far can it, or should it, be pressed, before humans cease to exist in a recognisable form? And is there something irreducibly human which will resist the ambitions of the engineers of the soul?

A better map

The two main problems we have identified in this book are related: insufficient generality of premises (epistemology) and lack of institutional mapping (ontology). We need a science which is more modest in its epistemology and richer in its ontology.

The parable of the blind men and the elephant (see above, p. 6) can be improved by constructing the following grid. On the vertical axis we plot ontology – the theory of what exists; on the horizontal axis, epistemology – the way true beliefs are generated.

Economics mainly occupies the top right-hand quadrant; sociology, politics, and history occupy the bottom left-hand one; psychology, the top left-hand quadrant. This leaves the bottom right-hand quadrant to historical materialism (Marxism). The argument of this book is that economics should move in the direction pointed to by the arrow, with less tight priors and looser deduction. It should be content, that is, with a logic of partial, rather than full, predictability.



8. Different Approaches to Understanding.

The further task is to link ontology and epistemology in a broader understanding, in which the economy is seen not as a specialised activity, with its own logic of behaviour, but as an aspect of human life and human striving. Polanyi expressed this idea in the view of the market economy as an embedded system.

The standard objection to broadening the scope of economic analysis in the way I have suggested is that it will make the subject too vague to be useful. This is Professor Krugman's view. He gives two reasons: first, thinkers, however eloquent, who adopt a 'discursive, non-mathematical style' will not be listened to by other economists; second, that 'controlled, silly models' are the only way to get at useful truths. The first is simply a

statement of current economic fashion; the second deserves more consideration. My argument is that the 'controlled, silly models' destroy old knowledge as much as they create new knowledge. This is because anything which can't be modelled in tight, silly ways is left out of the account. One can airily write off the destruction as the price of progress. But the resulting deficit in understanding may easily produce bad policy. In Krugman's own examples, the fact that economists couldn't model increasing returns to scale or oligopolistic competition till the 1970s (can they now?) meant they were stuck with the 'silly' model of the competitive economy.

I doubt if Krugman has realised the full import of saying that the methodology of economics prevents economists expressing 'sensible ideas'. His almost casual get-out is that in the long run these sensible ideas will be captured in 'fully worked-out models'.[8] But how long is the long run? How much useful knowledge is lost in the short run? And why on earth does he believe that even in the long run greater rigour will produce greater truth?

In the social sciences, formal modelling is unique to economics. Psychology, history, sociology, ethics do not rely on 'controlled, silly models' to get a better understanding of human behaviour. They aim at what Rosenberg has called 'qualitative', not 'quantitative', predictions. This is not a sacrifice mainstream economics has been prepared to make, for it would mean sacrificing its claim to be like a natural science. This would be fine if economics really were a natural science, if the policeman, decked out with his fancy equations, really did have the authority he claimed. But if economics is much like other social sciences, able to offer qualitative, not quantitative predictions, the claim that formal modelling is the only way to get at the truths which matter for economic life is a sign of hubris.

The radical question raised by Tony Lawson (see Chapter 7) is that if the material studied by economics is the same as the material studied by the other social sciences, what reason is there for the disciplinary divide between economics and the other social sciences, or indeed what would be the objection to a unified social science?

One answer is that the material of economics does exhibit 'closed worlds', absent from other social sciences, where quantitative predictions are to be had. These closed worlds are like the world of games, in which the

aims are given, the rules are fixed, and there is only a limited number of moves. They have always existed and exist today. They are the stuff of microeconomics. But I doubt if closure is a good general presumption to make of modern economic life, especially one dominated by financial institutions. The question which needs to be asked is: to what worlds does the study of economics add unique value, to what worlds does it add about the same amount of value as do other social sciences, and to what worlds does it add no value at all, and even detract from it?

Finally, we must return to a question central to pre-modern thought, but pushed aside by ‘scientific’ economics: what is wealth for? Ethics should be reinserted onto the ground floor of economics. By taking wants as given, economics offers no critique whatsoever of the human hunger for accumulating wealth without limit. That this might sanction policies which lead to the destruction of the human species is not something that someone who is just an economist need concern himself with. But a well-educated economist will surely have to do better than that.