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Economics as practical wisdom

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Abstract
The discipline of economics has been represented as deductive and theoretical, deductive and empirical, and inductive and empirical. All of these approaches have been subject to withering criticism in other social sciences: their weaknesses are theoretical, evidential and methodological. Part of the problem rests in the interpretative or prescriptive character of much that is written in economics, but the core rests in the attempt to generalise about economic processes beyond the context where they occur. The situation where an economic insight is applied cannot be bracketed off, or set aside, from its context and the influence of other factors; the process of generalisation does not of itself provide useful prescriptions for policy. The principles of phronesis are based in experiential knowledge and practice. Economics has to adapt to complexity and ethical considerations, relying on judgment in particular contexts rather than generalisation. It is a phronetic activity.

The discipline of economics is often misrepresented by its practitioners. Many of them see it as a theoretical, deductive science: Milton Friedman for example argued that economics offered hypotheses that could be tested, as physics does (Friedman, 1953). But there are many kinds of knowledge, and the approaches that characterise economics in the real world have far less to do with theoretical science than they do with practical judgment. Aristotle distinguished three different approaches to knowledge. There was scientific knowledge, based in theoretical reasoning, which he called *episteme*. There was *techne*, technical know-how, which identified the applications of knowledge and the process of acting on them. And then there was *phronesis*, or practical wisdom – a combination of experiential knowledge and judgment, based on understandings of the ways that things turn out in practice (Aristotle, *Ethics,* Book 6, ch. 5).

Economics is an exercise in phronesis. It is all about judgment – reading the situation, identifying patterns, applying conflicting principles, anticipating problems, spying opportunities. There are no certainties; there are tendencies, warnings, prescriptions and the recognition of trends or patterns, all subject to many reservations. I work in the field of policy analysis, a field where there have been persistent and intractable problems in applying any of the conventional methodologies of social science to policy development. In the process of trying to find a way of reconciling theory with practice, I came to realise that the accounts I was giving of policy analysis – accounts based in a different kind of methodology – applied with equal force to the methodology of economics. This paper is the result.

On economics as a science

Much of social science has come to rely on a set of analytical approaches intended to identify specific influences within a range of confounding factors. The object of analysis is to separate

the strands, to distinguish different influences, to attribute influence to particular variables. This has to be done in the face of multiple, competing influences.

As a discipline, economics goes about this in different ways. The traditional approach of classical economics is theoretical and deductive. Economic reasoning typically depends on theorisation about patterns of behaviour, subject to the assumption that 'other things are equal'. A range of models have been developed as a potential guide to analysis. Once the initial outline of a model has been determined, it is then possible successively to test the assumptions to see what the implications are of each further variable. This is the approach of much of what is done in microeconomic theory, including models of the actions of rational consumers or the behaviour of producers. Friedman argued that the test of theory was instrumental:

> "Viewed as a body of substantive hypotheses, theory is to be judged by its predictive power for the class of phenomena which it is intended to 'explain'.... To be important... a hypothesis must be descriptively false in its assumptions... the relevant question to ask about the 'assumptions' of a theory is not whether they are descriptively 'realistic', for they never are, but whether they are sufficiently good approximations for the purpose in hand" (Friedman, 1953, pp. 8, 14).

A second approach is empirical and deductive. Hausman refers to it as 'positivist' or 'Popperian' (Hausman, 1989); there are differences between those categories, but they share roots in empiricism rather than a primary dependence on theoretical reasoning. Explanations for behaviour are progressively refined through a process of conjecture and empirical refutation. This is the route that has been followed in the new 'behavioural economics', a fusion of psychology and economics. Psychology uses empirical situations, often controlled through experimental formats, to define and test the influence of specific constructs on behaviour. So, for example, the behaviour of a consumer faced with choices of what to purchase can be empirically constructed and examined (e.g. Sippel, 1997; Ariely, 2008). The shift to behavioural economics is viewed by many as a release from the limitations of traditional theorisation – and possibly exoneration from the charge that economic theory is too remote from empirical evidence to be scientific. (Syll is sceptical that there has been genuine movement. Many of the experimental conditions are still remote from real situations: Syll, 2016, pp. 148-152.)

The third approach is empirical and inductive. Econometrics typically proceeds by modelling the behaviour of an economy. This can be done through a deductive method, but more typically modelling is done by identifying patterns and trends inductively – using the data to construct descriptive equations. The process is designed to consider both the contribution of specific variables and their interactions.

Those who want economics to be more like the natural sciences sometimes argue that the weakness of economics rests in its over-reliance on theoretical deduction, and an insufficient emphasis on the empirical elements (Joffe, 2011). Criticisms of the methodology of economics have tended to focus on the limitations of economic models (e.g. Marques, 2015), the extent to which theoretical models can be used to explain empirical phenomena (Syll, 2016) , or the need to strengthen the subject's empirical basis (Blaug, 1992). In other fields of social science, however - including sociology, political science and social policy - the basic

principles behind all of these approaches have been challenged (Flyvbjerg, 2001; Laitin, 2003; Schram, 2003; Spicker, 2011). It is open to question whether social science can ever legitimately be based on idealised, causal or generative explanations. The influence of individual variables cannot confidently be isolated from the context in which the variables operate. Even if the limitations are overcome, it is not clear that it is possible to develop prescriptions for policy on this basis.

Episteme: economics as scientific knowledge

Most discussions of economic methodology focus on economics as scientific knowledge. *Episteme* was concerned, for Aristotle, with universals – general principles that could be discerned and understood. For some this might be taken to refer to certain knowledge, but certainty is not something that is fully compatible with empirical science, natural or social. It mainly relates, rather, to theoretical knowledge. While there are many competing views of science – for example, objectivist, positivist, instrumentalist, falsificationist, Kuhnian and realist (Chalmers, 1999) – most contemporary accounts of scientific reasoning share a basic appreciation that there needs to be some relationship between theoretical understandings and empirical evidence. Theory shapes the identification of evidence, and evidence shapes the development of theory.

Generalisation about empirical evidence is mainly based on the idea that what is true in one set of circumstances will also be true in similar circumstances elsewhere. That sounds, on the face of the matter, like the principle of 'induction', the assumption that things that happen together will happen together again. The presumption behind generalised empirical propositions of this type is not just that things happen together; it is that the observations stand in some kind of structural relationship to each other. That may be true because of the character of the relationship: if Y = C + I, I goes up and Y is constant, then C must go down (or there is something wrong with the equation). More typically, it is because there is a generative or causal relationship. Possibly this means that one factor causes another: excess supply leads to reduction in planned investment, a reduction in planned investment leads to lower aggregate activity. Possibly it means that the two factors are associated because of a common underlying cause, or set of causes: youth unemployment generally goes up at the same time as adult unemployment, because both reflect the demand for labour overall. Induction rests, then, on the same principles as deductive science. Induction and deduction share a sense that causal or generative processes make outcomes predictable, and that if we know the cause, the effect will follow unless there is a good reason to the contrary.

Conventional economic analysis invites economists to begin by setting the context aside, to proceed on the basis that certain principles can be applied as if "others things are equal". Economists may learn, for example, that

- people respond to incentives and disincentives
- beyond an optimum point, a firm's average total costs will increase with scale
- the demand for money increases when interest rates fall
- inflation increases when the money supply increases.

Although the format of these statements is similar, there are important distinctions between them. The first statement, on incentives and disincentives, is naked theory. It can be read as

a tautology (incentives and disincentives are what people respond to), as a hypothesis, or as an observation. However, taken as it stands, the claim, that people respond to incentives when other things are equal, is incomplete, which means that it cannot be directly applied. An incentive is (by definition) not just an inducement: it is an inducement that affects motivation and behaviour by offering a prospective gain. It makes no sense to say that people will respond to an inducement if there is no consideration of the attendant costs. Unemployment benefits are not an 'incentive' to become unemployed, any more than a longer prison sentence at the state's expense is an 'incentive' to crime or a death grant is an 'incentive' to become dead. That's not a fundamental criticism of economic principles. A generalisation about economic choice which disregards whether choices are eligible is not good theory.

The second statement is part of a standard model of production. It points to some useful observations: that firms have to choose how much to produce, that there may be limits to capacity and diminishing returns to scale, and that productive efficiency is not equivalent to maximisation of productive effort. However, it has been trenchantly criticised. Joffe argues that it is false for much of the time; it applies only to a minority of firms in practice.

> "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 ... firms do not recognise it as a description of what they do" (Joffe, 2011, p. 8).

The third statement, that the demand for money increases when interest rates fall, is a straightforward extension of the standard theory that demand responds to price, but it is not a theoretical statement: it is an attempt to describe what happens in the economy. The generalisation is certainly defensible in the abstract, but there are many circumstances where it would be wrong; and while there may be people who will agree with the statement in the round, it is not really sensible to accept it as it stands. There is the problem of definition: what is 'the' interest rate, what is the demand for money? Then, possibly more obvious, there is a problem with the assumption that other aspects of the economy can be bracketed off, so that (for example) the value of money, the state of production or the level of savings and investment are all irrelevant to the dynamic. And it is also important to consider the relationship between interest rates and policy – falling interest rates may be a consequence of falling demand.

The fourth statement, that inflation increases when the money supply increases, has all of these defects and more. It is incomplete: if inflation is 'too much money chasing too few goods', it is not enough to consider the money supply, because it also matters what is happening with production. The terminology is questionable. It is not possible to interpret the proposition in isolation from other factors; policy cannot be ignored. Beyond this, however, the statement has one further characteristic that sets it apart. The writers who adhere to this statement most strongly see the money supply as fundamental to the explanation of inflation (and are likely, as a result, to reject the reservations I have just made). This is not just about analysis; it is the underpinning of a system of values, a matter of ideology and of belief.

There are clearly problems in representing these statements as scientific generalisations. The problems include lack of clarity, the over-statement of certain positions and the intrusion of

ideological beliefs. More fundamentally, however, there are weaknesses in the very idea that such generalisations can be made validly and consistently with the empirical evidence. The first weakness is *theoretical,* reflecting the difficulty of attributing rationales to observations about association. Philosophers have been suspicious of arguments based on causation for a long time; Hume argued that as relationships of cause and effect could only be identified empirically, it could never be possible to prove anything more than a prior association (Hume, Book 1, III, chs 14-15). Sociology has tended to avoid this kind of generalisation since its phenomenological turn, some fifty years ago. The difficulty here is partly that links can be broken, but more fundamentally the associations that people believe must hold – such as, in the past, the classical assumption that frugality was the route to economic stability, Ricardo's 'iron law' that wages must always tend to subsistence levels, or the assertion that unemployment and inflation are inversely related – are conditional and contingent. Whether it happens because the theory is misconceived, or because the relationships are dependent on context, the associations that people identify may prove to be the wrong ones.

Another set of problems is *evidential:* it is difficult to know what constitutes evidence for a generalised proposition, let alone to provide evidence that is conclusive. This reflects the difficulty of describing variables and their associations appropriately. Many of the key variables in economics – growth, demand, unemployment or investment – are constructs based in aggregates, which disguise a heaving mass of contrary trends. ("There is no such thing", Hayek once wrote to Friedman, "as *the* quantity of money": cited Green, 1987, p. 148.) Associations, in a complex, often chaotic environment, come and go. Wherever there are multiple variables, there is always a possibility of spurious association; the tests we use are based on probability, and wherever there are large numbers of variables, there is the possibility that some may be correlated meaninglessly. False positives and false negatives are built into the statistical processes (see Ioannidis, 2005).

A third set of problems is *methodological.* What happens in principle when a variable is identified is that its effect is gauged against the influence of other variables. This can be done, for example, by distinguishing a control group – examining the circumstances when one group is subject to the variable and another is not. Many statistical procedures exist to replicate this effect. The central principle is that the influence of an independent variable can be bracketed off from other dependent variables, so that its effect can be considered in isolation. This situation is trenchantly criticised by Pawson and Tilley in their book, *Realistic Evaluation.* Pawson and Tilley are criminologists; their task was to identify the impact of certain interventions on crime. They had been using control trials to determine which methods were effective, but were tending to find that as the measures were rolled out more generally, their effectiveness seemed to diminish. The problem, they conclude, lies in the method itself. The purpose of analysis was to determine what the influence was of a particular effect when it was considered independently of its social context, and that was the opposite of what they really wanted to know. "Our argument", they write, "is that precisely what needs to be understood is what it is about given communities that will facilitate the effectiveness of a program! And that is precisely what is written out" (Pawson, Tilley, 1997, p. 52). The four statements considered in this section share the same flaw: all four are over-generalized. They lose their meaning when viewed in isolation.

Here is an example of an argument where the instrumental application of theory has been taken for granted. Rent controls are often held up as an example of the way that supply depends on prices; rent controls are supposed to limit the supply of rented housing by

restricting the price (e.g. Minford, Peel and Ashton, 1987; Albon and Stafford, 1987; Krugman, 2000). This is not what happens. Rent control is most practised in Northern European countries with extensive rented sectors; controls are less stringent in countries with smaller rented sectors, including the US and UK. The supply of rented housing has generally tended to be greater where rent controls are in force (Arnott, 1995). Anyone who wants to hold to the standard model needs then to qualify their argument to take account of the inconsistencies. The sources I have cited do the opposite, and simply fail to address the inconvenient contrary evidence.

Part of the explanation rests in the incompleteness of the theory. The income from rented housing does not consist solely of income from the rent paid by tenants. It also depends on the capital value of the rented housing, partly because historically capital accumulation has been a major part of returns to capital investment, and partly because it is the relationship of the rent to the capital value which determines the rate of return. Landlords do, in most economies, have other places to put their capital. So rent is not decisive, or even what matters most. The other part of the explanation, however, relates to the context where the principles are applied. The capital value of residential property in Britain has been overwhelmingly determined by its value to owner-occupiers. That has been true since the 1920s. Rental housing declined in the UK because the demand was diverted to alternatives. By the 1920s, owner-occupation was both more secure and cheaper for those on higher incomes. The construction of two million council houses between the wars removed much of the market for working-class housing; urban renewal from 1930 onwards demolished older private rented housing (see e.g. Homans, 1987); low returns led to limited investment and reduced maintenance. By the 1950s, the private rented sector was in steep decline, with a dilapidated stock and limited market. The landlords' best option at that stage was to realise the capital value. The effect of removing controls in 1957 was consequently not to revitalise the sector, as the government imagined it would, but to hasten landlords' exit from it (see Lansley, 1979; Cmnd 1246, 1960). Controls had to be reintroduced seven years later to stem the abuses which followed. A later abolition of most rent controls in 1988 had no immediate effect on supply (DCLG, 2016). Subsequent increases in the rental stock, especially since 2007, largely reflect changes in the rates of return, though there are other complexities in the market relating to the benefits system, the transfer of public housing stock between sectors and the treatment of rent and capital gains for the purposes of taxation.

Now, it could be argued that this is still an example of deductive reasoning, and the main problem it shows is that economists are not very good at revising expectations which are incompatible with the evidence. I think the flaw runs deeper than that. Modern economies rely on a complex, interrelated tissue of relationships; they are poorly described in terms of isolated trends divorced from other influences. The example illustrates the general point that the situation where a theory is applied cannot be bracketed off, or set aside, from its context. The methodology is not capable of handling the demands that are being made of it.

Techne: Economics as applied science

Techne is the application of skill or craft. Knowledge or episteme can be used to explain phenomena; *techne* is about how things are done. The practical application of knowledge is based, in the Aristotelian model, on universality (or generalisability) of experience; on its teachability; on its precision, or specificity; and a concern with explanation (Nussbaum, 2001,

pp. 161-2). Colander appeals to the metaphor of engineering: he sees the task of an engineer in terms of problem-solving, applying general theoretical insights to practical issues. Economics needs a similar focus on the real world (Colander, 2013, p. 58). The profession of economics, Sachs suggests, is 'clinical': it is there to cope with practical problems (Sachs, 2005). Both those comparisons represent economics as generalisable, analytically specific and able to guide action in practice. Nussbaum writes:

> "A person who says... that practical reasoning should become a *techne* is likely, then, to be demanding a systemization and unification of practice that will yield accounts and some sort of orderly grasp; he will want principles that can be taught and explanations of how desired results are produced. He will want to eliminate some of the chanciness from human social life" (Nussbaum, 2001, pp. 162-3).

Some economists want more than that: they are trying to use economic theory to build a blueprint for policy. The 'Washington Consensus' refers to an economic orthodoxy – ten central economic propositions about policy that were shared by most serious economists working with developing economies. The consensus was based on:

- fiscal discipline
- reordering public spending priorities
- tax reform
- liberalising interest rates
- competitive exchange rates
- liberalising trade
- liberalising foreign direct investment
- privatisation
- deregulation, and
- the establishment of property rights (Williamson, 2000).

None of these policies, singly or together, gives any guarantee of economic development. We can be reasonably sure of that, despite the apparent agreement to the contrary, because we know that structural adjustment introduced on these principles failed to deliver. Of 29 countries which underwent structural adjustment in sub-Saharan Africa, six were judged by the World Bank evaluators to experience an 'improvement' in policies – not in outcomes – nine a small improvement, and eleven were worse than when they started (World Bank, 1994, p. 3). It may be argued, of course, that none of these countries succeeded in implementing the policies that the orthodoxy recommended – that has been the defence of ideologues through the ages – but what use is a policy that no-one is capable of implementing? After structural adjustment fell from favour, the international organisations started to develop a different approach to policy – policies based on dialogue, deliberation, partnership and self-direction. Those policies have been remarkable in their effects, in terms of growth, improvements in welfare and legitimacy (Radelet, 2010).

This is not so much a failure of economic methodology as a reflection of its application to a task it was never designed to do. The methodologies I have been considering principally work by theorising, analysis and modelling, so that we can explain or at least model the influence of different elements and show the direction and magnitude of different influences and effects. Friedman writes:

> "Any policy conclusion necessarily rests on a prediction about the consequences of doing one thing rather than another, a prediction that must be based – implicitly or explicitly – on positive economics" (Friedman, 1953, p. 5).

Policy might depend on prediction, but it does not need to: it might depend on values, on incremental change, on accepted patterns of behaviour, even on 'muddling through' (Lindblom, 1973). Many people seem to think that once we know the way into a problem, we will better be able to find the way out of it. This is an obvious fallacy: if we fall down a mine shaft, what we understand about the principles of gravity is going to do very little to get us out. We may know, or think we know, that profligate public spending can lead to inflation; it does not follow that retrenchment will reduce it. We may think that increasing the price of labour reduces employment (that is very disputable, because participation in the labour market tends to rise with median income), but it does not follow that reducing wages increases employment. We may think we know that growth can be depressed by restrictions on private sector development; it does not follow that deregulation will produce growth. If this limits the application of economic insights, that is because those insights are much more concerned with explanation after the event than they are with prediction or policy. A plausible analysis might help to avoid some further traps, but it is not a blueprint for action.

Although Friedman referred to economic theories as 'hypotheses', he sees them as tools, rather than hypotheses that are likely to be rejected when they do not apply (Friedman, 1953, pp. 14-5), and he did not draw any clear distinction between the 'hypotheses' of economic theory and prescriptions for policy (e.g. Friedman, 1962). Keynes wrote:

> "Economics is a science of thinking in terms of models joined to the art of choosing models which are relevant to the contemporary world... Good economists are scarce because the gift for using 'vigilant observation' to choose good models, although it does not require a highly specialized intellectual technique, appears to be a very rare one" (Keynes, 1938).

This reflects the impossibility of dealing with the confounding factors and complexities. Sometimes economic models can be applied directly to empirical data. Sometimes they cannot. Unfortunately, economics as a discipline is not very good at telling us which is which.

Economics and phronesis

Phronesis is often translated as 'practical wisdom'. Aristotle explained:

> "It is not science, because conduct is variable, and it is not art, because doing and making are different in kind. Practical wisdom is a rational faculty exercised for the attainment of truth in things that are humanly good and bad" (Aristotle, *Ethics,* Book 6 ch 5, p. 177).

Aristotle favoured a process of reasoning, sometimes identified with the idea of a 'practical syllogism', where actions can be justified in practice through general propositions and a process of deliberation to explain how decisions are taken in specific situations and review.

The philosophical literature on 'practical reasoning' goes beyond this, straddling issues in the philosophy of mind, ethics and rational choice theory (Raz, 1978; Millgram, 2001). Phronesis is only part of this constellation; understanding the whole process of rational deliberation calls for consideration of all the elements of knowledge and their relationship to each other. Practical wisdom calls for the translation of principles to particular circumstances, but putting it that way has it the wrong way about; phronesis begins by considering the particular circumstances, and goes from there to identify what kind of response is appropriate to those circumstances.

Noel identifies three discrete, and sometimes competing, accounts of phronesis: rationalist, moral and situational (Noel, 1999). The first of these categories may be surprising to those who think – despite Aristotle's explicit reference to a 'rational faculty' – that phronesis is always normative, or a departure from rational thinking. The rationalist view of phronesis looks for precepts – generalised guidance about the principles that apply, based on practical experience - and a deliberative process of application, so that an account can be given of the reasons for a decision in a particular situation. Phronesis is sometimes rendered in translation as 'prudence' (a term Nagel identifies with 'practical foresight': Nagel, 1978, p.159): prudential decisions extend consideration to the future, calling for anticipation, safeguarding and a degree of flexibility. This all seems to square with Keynes' view of economics. Keynes favoured the use of generalised models, but he was sceptical as to how generally they could be applied. He wrote that that "…the material to which (economics) is applied is, in too many respects, not homogeneous through time" (Keynes, 1938) – a phrase which suggests that empirical results in economics are contingent and not replicable.

The moral understanding of phronesis emphasises its normative character. The reference to what is 'good' or 'bad' can be read both as a normative statement and as a description of what happens. That parallels a long-standing debate in economics about positive and normative methodology (Weston, 1994). Economics, Keynes wrote, is essentially a moral science; the point has been taken up by Tony Atkinson (Atkinson, 2008). By some lights, phronesis is primarily moral; it is about what should be done (Rooney and McKenna, 2006). Aristotle did not intend the idea to be exclusively normative, however – he also emphasised the empirical and experiential aspects.

> "Intelligence apprehends the truth of definitions which cannot be proved by argument, while phronesis involves knowledge of the ultimate particular thing, which cannot be attained by science but only by 'perception'" (Aristotle, Book 6 ch. 8, p 182).

The situational view of phronesis focuses on the particular nature of decisions. The kinds of question which phronesis attempts to answer, Flyvbjerg suggests, are "Where are we going?", "Is this desirable?" and "What should be done?" (Flyvbjerg, 2001, p. 60). Situational views base phronesis in terms of the context where the issues take place. The key question is not, "What should I do?", but "What should I do in this situation?" (Noel, 1999, p. 274). That comes fairly close to Colander's understanding of applied economics (Colander, 2013) – probably closer than his own analogy with engineering.

Phronesis has three key characteristics. First, it is always empirically based – it cannot be arrived at by a priori thinking, because that is knowledge of a different kind. Second, whereas episteme is universal, phronesis is particular – it is dependent on circumstances and context.

Third, it is always equivocal, and it tends to be approximate; there are exceptions to nearly every generalisation.

Examples of statements that are phronetic, rather than epistemic or technical, might be

- that demand tends to fall when prices increase
- that competition tends to promote diversity or
- that over time, people tend to demand more of a commodity than they did.

All of those statements have been presented at times as if they were the product of deductive scientific reasoning. They are not. None of the statements is universally true – there are many exceptions, even when other things are equal. None of them can validly be considered independently from the circumstances where it occurs. But they are all useful phronetic generalisations. They are derived from practical experience. They work much of the time. They are *more or less* true. And that – rather than any claim to deductive science - pretty much sums up the way that economics works in practice.

Friedman's instrumentalist argument depended on establishing 'hypotheses' that make for 'good approximations'. Many theoretical statements in economics are not approximations, and are not derived from practical experience. They are not 'hypotheses' at all, in the sense of being generalisations which are then to be subject to empirical test. Consider some commonplace propositions from the theoretical literature:

- rational individuals maximise their utility (Winch, 1971)
- self-interest means that co-operation must break down (Hardin, 1968)
- 'Consumers have unlimited wants. They would always prefer to consume more rather than less' (Anderton, 1997, p. 83).

These propositions fly in the face of practical experience. There is no evidence to show that people maximise anything; some cooperative arrangements have lasted for centuries; people's tastes change when their incomes change, and they don't just go for more, they go for something different. The statements are not 'more or less' true; they are not true at all. They are not even minimally plausible.

Why, then, do people accept them? There may well be some economists who don't care whether economics is true or not, so long as it is internally consistent (e.g. von Mises, cited in Blaug, 1992, p. 80) but I venture to suggest that this is not the general view. These propositions continue to exercise their sway because each of them stands for something different from what it might appear to say. The first proposition disguises a prescription. People may not be rational, and they may not maximise their utility, but if they wanted to achieve aims more effectively, an analysis based on maximisation would give them some guidance. The second one carries a warning. People are not exclusively self-interested, but if they are to cooperate, the analysis tells us that some provision needs to be made to restrain self-interest and free riding (Olson, 1971).

The third proposition, the principle of non-satiation, is the most interesting, because it is predictive, not simply prescriptive – and there is a family resemblance to one of the examples I gave of a phronetic generalisation, that consumption tends to increase over time. People do not have unlimited wants – the very idea that they do is absurd. Many commodities come at a

cost, and having too much of something can be a curse – such as too much hospital care (a big problem for older people). Some economists try to exclude the possibility of over-consumption by referring to 'local nonsatiation', which is a marginal concept – non-satiation within the immediate confines of a situation – but that does not work either; even within the restricted scope of a laboratory experiment, people do not consume without limit (MacCrimmon and Toda, 1969). The value of analyses based on nonsatiation is partly that it can be used to think about behaviour in the immediate context where demand is not satisfied, and partly that it can be applied to other conditions in the longer term (though that is more debatable, because changes in long-term preferences suggest a shift in the function rather than an extension of it). It is probably true, as a matter of observation, that people often do tend to demand more of a commodity than they did, subject to substitutes and relative costs; over time, the demand for health care for older people has progressively increased, even if the care is no longer done in long-stay hospitals. Viewed in context, nonsatiation becomes an interpretative principle, rather than a universal truth.

I have given six examples of generalised propositions in this section. Taking them together, it is difficult to argue that they are hypothetical, that they are derived from theory rather than assumption, that the line of reasoning they follow is deductive, or that they can usefully be tested against 'the facts'. That is not what they are there for. The approaches they embody – observation, prescription, warning and interpretation – are characteristic of phronesis rather than theoretical science or technical knowledge.

Economic reasoning depends heavily on generalisations that are necessarily contingent and provisional. Supply may tend to increase when prices go up – but there are so many other factors in a supply function that it is not possible to assume that higher prices alone will have that effect. The division of labour increases the potential consumption - but the impact of specialisation may reduce the capacity to consume. Inflation happens when too much money chases too few goods – but inflation can also affect the supply of goods. There is hardly a proposition in the field that can be applied in practice without reservation. That does not make such statements wrong, or useless, but it does change their status. Economic theory is not, and cannot pretend to be, a description of how people will act. It might be seen as an attempt to construct a narrative, or to describe patterns, which may or may not give some insight into the interpretation of complex social situations (cf Blaug, 1992). Whether the pattern offers such an insight depends on the situation it is applied to. That is the application of *phronesis*, not of *episteme* or *techne*. Economics is a phronetic activity.

There are good reasons to be sceptical about many of the methods used in theoretical economics, but it does not follow that economists need to abandon them. The purpose of this paper, rather, is to encourage economists, regardless of their political perspective or theoretical orientation, to recognise what they are actually doing, and to invite a reconsideration of how economics ought to present itself. Economics is not based on deduction. It does not proceed by empirical examination of hypotheses. Nor does it proceed by inductive generalisation of empirical findings. Economic analysis is phronetic by reason, by temperament and by practice. It has to cope with complexity, context, with ethical imperatives. It is notoriously true that economists don't agree in their analyses or predictions. Nor should they. There is nothing wrong, and much right, with a statement that says "…this is a possible outcome, and this is what experience suggests, but there are contradictory pressures, and it might not work out like that when we try it the next time." That is not talking like a theoretical economist, but maybe it should be.

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