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On the Limits of Rational Choice Theory

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ABSTRACT

The value of rational choice theory for the social sciences has been long debated. Such rational choice theory involves a theory of behaviour based on the assumption that individuals are acting, or acting as if, to maximise their utility. The critique developed here focuses on the universality and unfalsifiability of the rational choice approach. In principle it can be adapted to fit any form of behaviour, including the behaviour of non-human organisms. Rational choice theory has the character of a universal 'explanation' that can be made to 'fit' any set of events. This is a sign of weakness rather than strength. Powerful explanations in the social sciences must focus on the particularities of the human and modern condition. A theory that brings in those particularities as an afterthought will fail to capture their importance. It is shown that key concepts such as culture and learning fall into this category. The problem with rational choice theory is that, in its excessive quest for generality, it fails to focus on the historically and geographically specific features of the socio-economic systems. As long as social theory is confined to generalities, then it will remain highly limited in dealing with any specific world, including the one in which we live.

On the Limits of Rational Choice Theory

by Geoffrey M. Hodgson¹

The question of the scope and validity of rational choice theory remains central to the discourse of modern social science. There is widespread agreement that, at least within some finite range of 'bounded' possibilities, some limited notion of rational choice may pertain (Simon, 1957). While problems then remain over the nature of 'bounded' rationality, the most important controversy concerns the much wider extension of rational choice theory to multiple domains, and its claims of explanation in this broader context. By rational choice theory in this context, we mean a theory of behaviour based on the assumption that individuals are acting (or acting as if) to maximise their utility. Increasingly recurrent claims have been made that this theoretical approach is capable of embracing key aspects of social as well as economic reality (Coleman, 1990; Coleman and Fararo, 1990). Indeed, approaches based on rational choice have escaped from their previous confinement within mainstream economics and are now making enormous inroads into sociology as well as political science.

Against this trend, leading social theorists such as Amitai Etzioni (1988) have argued that rational choice theory omits unavoidable ethical dimensions of social or economic life. Amartya Sen (1976) has argued concordantly that the picture of the individual in exclusive pursuit of self-interested utility denies the possibility of personal commitment to social or other values. Sen's and Etzioni's critiques are well-taken; they point to the limits of a purely utilitarian analysis of human behaviour that lies at the foundation of rational choice analysis. Further development of this type of critique involves a substantial excursion into philosophical territory, and discussion of the failure of utilitarianism to acknowledge ethical issues that are incapable of reduction to questions of individual satisfaction or utility.

The critique presented here is of a different type, but it is complementary to some of the critiques that have focused on ethical and philosophical issues. The arguments here are not yet prominent in current debates, but they have precedents in the literature. Contrary to many of its supporters and critics, it is argued here that all claims of rational choice theory to 'explain' social or economic phenomena are strictly unfalsifiable, by any appeal to evidence. Accordingly, rational choice theory has the character of a universal 'explanation'. In principle it could be made to 'fit' any set of events, including the behaviour or organisms in the non-human world.

However, it is argued that the outcome of the universality of the theory is a sign of weakness rather than strength. A theory that applies to non-human as well as human

¹ The arguments in this paper were presented at the July 2000 meeting of the Society for the Advancement of Socio-Economics at the London School of Economics. Extensive use is made of material from Hodgson (2001). The author is especially grateful to Peter Abell, Mark Blaug and John Rogers Hollingsworth for discussions.

phenomena is obliged to omit characteristics that are specifically human. For rational choice theory this is a huge and damning omission.

As a result, rational choice theory fails to focus clearly on key human concepts such as culture and learning. While rational choice models have been developed that seem to encompass such phenomena, they fail to identify crucial aspects and become incapacitated by their over-generality.

The rise of rational choice theory

As the apparatus of rational choice theory has been both broadened and refined, its claims have expanded with increasing confidence, to the point where they lay claim to the territory not only of economics but to the entire social sciences and beyond.

Earlier neoclassical economists, such as Alfred Marshall and Vilfredo Pareto, made it clear that economics was concerned with the more deliberative and calculative aspects of human behaviour. Marshall (1949, p. 17) wrote that 'the side of life with which economics is specially concerned is that in which man's conduct is most deliberate'. Pareto (1971) saw economics as being concerned with 'logical' actions, namely those where means are logically related to ends. Pareto (1935) also devoted himself to the quite separate science of sociology, claiming that this, in contrast, dealt with 'non-logical' action. From both the Marshallian and the Paretian points of view, economics was not an all-encompassing social science. It was concerned with particular kinds of activity or behaviour.

Philip Wicksteed defined the domain of economics differently. He argued that the distinctive feature of 'an economic transaction is that I am not considering you except as a link in the chain' (Wicksteed, 1933, p. 174). In other words, economics was the study of relatively impersonal transactions. However, although the lines of demarcation were different, economics was still confined in its scope. A legitimate place was accorded to other social sciences.

Lionel Robbins (1935, p. 16) began to change things radically with his new definition of economics as 'the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses'. This forced sociologists such as Talcott Parsons onto a different tack. Sociology was to be the study of the formation of ends, economics of the means to attain given ends. A key difference in this new demarcation was that there was no longer a domain of social activity that was in principle free from the potential clutches of 'economics' as Robbins had defined it. Robbins explicitly denied that economics was concerned with specific areas of enquiry, such as money, prices and markets. The boundary in the real world between the 'economy' and 'society' was no more. Henceforth, economics and sociology were both to concern themselves with the analysis of all human activity: the former with its means, the latter with its ends.

It then became possible to separate completely the concept of utility from the idea of price or monetary value. Although money values could occasionally be used as surrogates for levels of utility (given the assumption of a constant marginal utility of money) this did not have to be so, especially in the field of pure theory.

Shortly afterwards, Paul Samuelson (1938) and others insisted that economics could base itself on the claims of 'revealed preference' alone, and did not need to invoke any

psychological theory of human behaviour (Lewin, 1996).² Sociology had broken with psychology, and mainstream economics rapidly followed suit. Like sociology under Parsons, mainstream economics then saw itself as independent of any psychological postulates. Demand theory was allegedly freed from both psychological theory and intersubjective interpretations of behaviour. The flirtation with revealed preference theory was crucial in this process. By focusing on measurable outcomes, it was believed that utility theory had become 'objective' and 'scientific'.

Similarly, the assumption of deliberate or conscious choice was also regarded as inessential and unnecessarily restrictive, and removed from the theory (Machlup 1946; Friedman, 1953). As Ian Little (1949, p. 90) remarked, as a result of these developments, 'a theory of consumers' demand can be based solely on consistent behaviour' rather than consumer propensities or plans. All that was required was that behaviour appeared to be consistent: in which case a fixed preference function could be imputed that would satisfy the standard axioms of utility theory.

Once the core axioms of mainstream economics were reduced essentially to 'consistent behaviour' then the door was open to the removal, not only of psychology, but also of real economic and social institutions from the picture. By the 1960s this process was largely complete. Not only economics but also sociology was affected by this widespread thrust towards general or universalising theories.³

The theory of 'rational choice' has been held up as the theoretical jewel in the neoclassical crown. It comes in various versions, but the central idea is that we may model individual behaviour in terms of a given preference function, in which agents maximise their 'utility'. This function specifies the amounts of utility yielded from each combination of specific inputs. Each input enters as an argument in this function. These inputs can be standard consumer goods or services but can in principle include other items, such as the 'human capital' of the consumer, or the utility of others, or the available 'social capital' (Becker, 1996). It is assumed that individuals make the 'rational choice' that maximises their utility according to the options available. The whole approach is to explain human behaviour simply on the basis of such preference functions, given limited resources and other constraints.⁴

Note that this general approach does not even tell us whether the individual will behave in a selfish or altruistic way. In his modern guise, rational economic man is not necessarily a selfish hedonist. The possibility of a type of 'altruism' is admitted because the individual may have a preference function that ensures that extra utility is gained from the enhanced utility of

² Although Samuelson's 'revealed preference' theory is now widely regarded as a failure (Majumdar, 1958; Sen, 1973; Wong, 1978), it nevertheless had a lasting effect, particularly by helping to break the surviving mainstream links between economics and psychology.

³ Mouzelis (1995, p. 5) complains of the surfeit of 'transhistorical, universalistic statements' in sociology which do not take into account 'history and context' and 'tend to be either wrong or trivial.' One of his prime exemplars is rational choice theory.

⁴ It should be pointed out that whether this theory does involve genuine choice is questionable. As Buchanan (1969) and several others argue, a choice is only meaningful if there is a possible alternative. We must have been able to 'act otherwise'. The utility-maximisers of neoclassical economics are more like programmed automata than real choosers.

others (Collard, 1978). This means that the giving of a gift can mean a net gain in utility for the giver: the loss of utility resulting from the loss of the gift is compensated by a gain in utility resulting from our sensitivity to the increased utility of the recipient. In this way, 'altruism' can be ostensibly 'explained'. Rational choice theorists do not have to confine their models to agents who are simply maximising their own assets.⁵

This relentless quest for universality gives rise to what is described by its practitioners as 'economic imperialism'. This refers to the invasion of other social sciences with the choice-theoretic methods of neoclassical economics. It is argued that the core assumptions of neoclassical economics can and should be applied to a wide variety of fields of study, including politics, public administration, sociology, anthropology, psychology, history and even biology, as well as economics itself. It is based on the belief that the idea of 'rational economic man' is appropriate to behavioural science as a whole. The case for the conquest of other social sciences and biology by neoclassical economists rests on the presumed universality of such ideas as scarcity, competition and rational self-interest.⁶

However, in their enthusiasm for economic imperialism, the advocates of the universal rational economic organism eventually settled on a definition of rationality that was unfalsifiable. The concept had become so elastic that any circumstance could fit it. This outcome is explored further below.

Making predictions

A widespread justification of utility theory is its alleged capacity to make predictions. Milton Friedman is a well-known exponent of this view. According to Friedman (1962, p. 13) 'economic theory proceeds largely to take wants as fixed'. The economist then makes predictions on the basis of this assumption. The legitimacy of this abstraction then allegedly rests on its 'power to predict'. This is a positivist and instrumentalist criterion of theory justification, in terms of the capacity to make predictions. Countless models have been developed on the basis of the utility maximising or 'rational' choice. Some of these models generate falsifiable predictions.

Consider the early attempts to apply utility-maximising, rational choice models to political phenomena. Some models in this vein predicted a zero turnout in democratic elections. The reasoning was as follows. With a sufficiently large number of voters, the costs of voting outweigh any positive marginal benefit to the voter, so there would be no net incentive to vote (Riker and Ordeshook, 1968). However, large numbers of people do vote voluntarily in elections. The prediction of the model is manifestly false.

⁵ However, the altruistic agent is still maximising his or her own utility. On the contrary, true altruism would occur if we gave to others and made ourselves worse off, even in net utility terms. This criticism shows that utility theory still depends upon a specific, self-centred notion of the individual and is not so universal as its advocates make claim. Nevertheless, this does not make the theory adequate for the analysis of a society of greedy or self-centred individuals, because the institutional and cultural bases of such a system are inadequately explored.

⁶ Prominent extensions to biology include Becker (1976) and Hirshleifer (1977, 1985). On 'economic imperialism' see also Radnitzky (1992), Radnitzky and Bernholz (1987) and the critiques in Nicolaidis (1988) and Udéhn (1992).

However, although the particular model is severely questioned, this evidence does not in any way refute utility maximisation or rational choice theory. It refutes one model only, which is based on specific and narrower *additional* assumptions. Subsequently, political theorists have had little difficulty constructing different rational choice models that generated predictions that got closer to voting turnouts in the real world. For instance, it could be assumed that people are getting a substantial amount of utility simply from placing their vote. Tune the utility function appropriately, and we get a closer approximation to the empirical data on real world behaviour.

The point being made here is not that rational choice or utility theory is either refuted or confirmed by the evidence. The point is that utility theory can be used to make falsifiable predictions, but *only* when specific *auxiliary* assumptions are made. As Mark Blaug (1992, p. 232) observed: ‘The rationality hypothesis by itself is rather weak. To make it yield interesting implications, we need to add auxiliary assumptions’. These add-on assumptions may concern the shape and arguments of the utility functions, the nature of the constraints, the existence of uniformities between agents, and so on. It is these *additional* assumptions that do the predictive work, not the assumptions of rationality or utility-maximisation *per se* (Shaper, 2000). By this argument, utility theory is not necessarily wrong. But it is manifestly inadequate. Utility theorists demonstrate these inadequacies themselves when they always have to bring in additional assumptions to make any meaningful empirical prediction.

The non-falsifiability of the theory

Perhaps a fundamental ‘predictive’ claim of utility theory is that the substitution effect is negative. The detailed argument can be found in the neoclassical textbooks. This shows that if a price increase occurs, and compensation is made for any change of ‘real’ (i.e. utility) income, then the demand for the good or service will decrease. Conversely, a price decrease will lead to a demand increase, under similar compensatory conditions. The proof of the negativity of the substitution effect follows directly from the assumptions of the theory (Hicks, 1939).

Can the negative sign of the substitution effect be used to predict human behaviour? Is it a falsifiable prediction? Regrettably, the answer to both questions is no. *Any observed behaviour can be fitted into the theory*. If the price increases and demand also goes up, then that does not contradict the theory. In this case it could simply be said that the ‘real’ income (measured in terms of utility rather than prices) is not constant. If we were to make an adequate income compensation, and assume a sufficiently lower ‘real’ income before the price change, then the apparent anomaly would disappear. We are free to make a wide range of assumptions concerning the imagined compensation. The compensation has to be such to place the individual at exactly the same utility level, before and after the price change. But we do not know this utility level, or the shape of the indifference curve!

The compensation is thus a thought experiment, rather than an investigation into processes in the real world. Reality can be no adjudicator in this thought experiment, because we cannot directly measure utility. This high degree of compensatory discretion makes the theory untestable in terms of its behavioural predictions. The result may have the aesthetic appeal and the apparent universality of a mathematical theorem, but it does not enable us to make any prediction that can be falsified by any possible outcome in the real world.

Experimental psychologists such as Daniel Kahneman, Paul Slovic and Amos Tversky (1982) have thrown down experimental challenges to expected utility theory. More broadly, since the 1980s there has been a spectacular growth in interest in ‘experimental economics’. Many people have interpreted the behavioural evidence gathered by the experimenters as a

violation of the standard axioms of expected utility theory. Much of this evidence, particularly concerning choices under risk, has led some mainstream theorists to reflect critically upon the standard assumptions of their theory. Evidence is important and it should be taken seriously.⁷

However, if we were to think that the evidence itself refutes or falsifies the core axioms of utility theory, then we would be mistaken. The reason being that the standard core of utility theory is *non-falsifiable*. As Sidney Winter (1964, pp. 309, 315) argued in an early and neglected article: ‘any behavior can in one way or another be rationalized as maximizing behavior’.

Lawrence Boland (1981) expanded on this in another important paper. With the provocative title ‘the futility of criticizing the neoclassical maximization hypothesis’, his essay was first widely misinterpreted as a defence of a theory that the mainstream economists had already accepted and taken for granted. Consequently, Boland’s paper is now largely forgotten.⁸

In fact, it is better understood as a *critique* of the maximisation hypothesis. In his paper, Boland asked if any conceivable evidence would refute the maximising assumption. He then showed that such an attempt at falsification could never work:

Given the premise – ‘All consumers maximize something’ – the critic can claim he has found a consumer who is not maximizing anything. The person who assumed the premise is true can respond: ‘You claim you have found a consumer who is not a maximizer but how do you know there is not something which he is maximizing?’ (Boland, 1981, p. 1034)

Given that we can never in principle demonstrate that ‘something else’ (perhaps unknown to us) is not being maximised, then the theory is ultimately invulnerable to any empirical attack. To show empirically that nothing is being maximised we would have to measure every possible variable that could impinge upon humanity, from the weather to the twinkling of the stars. Clearly, this would be an endless and impossible task. As Boland (*ibid.*) concluded:

The neoclassical assumption of universal maximization could very well be false, but as a matter of logic we cannot expect to be able to prove that it is.

Boland showed that the neoclassical assumption is not falsifiable. But he also rightly points out that it is not a tautology. It is not a tautology because it is *conceivably false*. It might be the case that nothing is being maximised. But we can never know.⁹

⁷ For summaries of the issues and debates in experimental economics see Kagel and Roth (1995). The debate is taken further by Binmore (1999), Loomes (1998, 1999) and Starmer (1999a, 1999b).

⁸ See Boland’s (1996) own later reflections on the misinterpretation of his argument.

⁹ Given that the theory is unfalsifiable, Boland (1981) goes on to examine its ‘metaphysical’ status. Here Boland can be misunderstood, unless his strong Popperian inclinations are acknowledged. He alleges that it is ‘futile’ to criticise the theory because it is ‘non-falsifiable’ and thereby ‘metaphysical’. By the famous Popperian criterion, this also means that it is ‘non-scientific’. This is the understated and impish outcome to Boland’s argument. Where Boland is vulnerable is not in the demonstration of unfalsifiability but in his excessive faith in the Popperian criterion. From this viewpoint, non-falsifiable statements are ‘metaphysical’ and invulnerable: trying to refute them is a waste of time. Hence, for Boland, ‘criticism’ can usefully be directed at falsifiable statements only – and the main means of ‘criticism’ is empirical falsification. In response, Caldwell (1983) shows that Boland’s demonstration of ‘the futility’ of criticising the hypothesis rests upon an overly narrow notion of

The arguments of Winter and Boland have been much neglected. They do not rule out the role of evidence in evaluating the theory, but they show that the evidence alone cannot be decisive. Boland also warns us that utility maximisation is not ‘tautological’. Strangely, some critics regard the allegation of ‘tautology’ as a damning weakness. On the contrary, a tautological theory, whether it is ‘empty’ or not, must be accepted as valid. By saying that utility maximisation is not a tautology we are admitting the possibility that it is false, although no single piece of evidence can show that it is untrue.

In some respects, Boland’s argument resembles the so-called Duhem-Quine thesis.¹⁰ This thesis derives from the work of the French physicist Pierre Duhem and the American philosopher Willard van Orman Quine (Harding, 1976). According to the thesis that is given their joint name, it is not possible to falsify a single hypothesis because we are always faced with a tangle of related and connected hypotheses. Consequently, we can never be sure that the main hypothesis is being targeted and tested on its own, and that other auxiliary hypotheses are not complicating the picture. Boland, Duhem and Quine all point to the multiplicity and interconnectedness of possible causal influences behind any empirical phenomenon in the real world, and the general difficulty of isolating and testing them all.

Just as we cannot isolate every connected and auxiliary hypothesis, we cannot consider all the possible hypothetical variables that could be maximised. As a result it can be argued that there is no experimental or other phenomenon that cannot in principle be ‘explained’ by the theory. Nothing lies outside its scope. Even the so-called anomalies revealed by experiments with human subjects can be explained away. If experiments show that some consumers appear to prefer a monetary reward that is less than the expected outcome, or appear to have intransitive preference orderings, then we can always get round these problems by introducing other variables.¹¹

For instance, if an experiment shows that option *A* with an expected value of \$4 is preferred to option *B* with an expected value of \$5 then we can simply assume that there are additional attributes of option *A* (for example, we may enjoy losing, or gain pleasure from seeing others win) that are consistent with the view that it yields higher overall utility for the subject. Likewise, an experiment may seem to reveal preference intransitivity, by showing that while *X* is preferred to *Y*, and *Y* is preferred to *Z*, *Z* is preferred to *X*. Even this result can be explained away by showing that the three pairwise comparisons did not take place under identical conditions, or were separated in time or space. Accordingly, the consumer could have ‘learned’ more about his or her true tastes during the experiment itself, or other factors may account for the apparent intransitivity. All we have to do is indicate in some way that the

‘criticism’. Caldwell argues convincingly that it is also possible to criticise particular non-falsifiable statements, for instance by looking at their underlying assumptions. Caldwell thus points to a wider and deeper discussion of the role and viability of core assumptions. In practice, the falsification criterion is not only too narrow but also potentially far too destructive for any theory. Strictly applied, just one apparently conflicting observation would serve to destroy the theory in question. Caldwell is right to suggest that the appraisal of theories must deploy a number of additional criteria, and not pin everything on falsification. Nevertheless, Boland’s central result – that no imaginable evidence can in principle falsify the theory – still stands.

¹⁰ Some of the implications of this thesis for macroeconomics are discussed by Cross (1982). Cross usefully reviews some of the attacks on the Duhem-Quine thesis and concludes that it has ‘withstood criticism’ (p. 322).

¹¹ Hausman (1992, ch. 13) documents several attempts to explain the apparent anomalies that have been revealed by the experimenters, notably by pointing to other possible sources of utility.

two Zs in the above comparisons are not quite identical. The two Zs could be slightly different in timing, substance, or their informational or other contexts. We then get the result: X is preferred to Y , Y is preferred to Z_1 , and Z_2 is preferred to X . Transitivity is no longer violated.

It is also claimed that preference reversals are inconsistent with expected utility theory. 'Preference reversals occur when individuals are presented with two gambles, one featuring a high probability of winning a modest sum of money ... the other featuring a low probability of winning a large sum of money' (Slovic and Lichtenstein, 1983, p. 596). Assume that a subject is faced with a choice between \$10 with certainty, and \$1,000 with a probability of 2 per cent. Experiments with real subjects indicate that in such situations the first, \$10 option is sometimes chosen (Kahneman *et al*, 1982; Slovic and Lichtenstein, 1983). This is despite the fact that the expected value of the second option is higher at \$20. However, preference reversals also fail to falsify expected utility theory, once we accept that utility is not necessarily measured in terms of the monetary payoffs in the experiment. If we assume a sufficiently declining marginal utility of money, or an added disutility associated with involvement in a risky and low probability choice, then expected utility theory is not overturned. In general, a risk averse actor will not maximise expected monetary value but still be maximising expected utility. By appropriate functional manipulation, the choice of \$10 can be made perfectly consistent with the maximisation of expected utility, rather than the maximisation of the expected monetary value of the payoff.

Experimental economists such as Vernon Smith (1982) and others have addressed the problem of the possible absence of a linear correlation between utility and monetary payoff. In particular, the possibility of additional, subjective utilities - unrelated to the monetary payoffs - has to be diminished. The money payoffs have to 'dominate' the decisions of the agents. To make experiments 'work' in the sense of a close presumed correlation between overall utility and monetary payoff, Smith proposes a number of 'precepts' of experimental assumption and design constituting an 'induced value procedure'. These precepts include nonsatiation, sufficiently large and obvious rewards, restriction of communication between subjects, and so on. But Smith (p. 929) himself is the first to admit that these precepts cannot guarantee any correspondence between observable monetary rewards and preferences that, in principle, are 'not directly observable'. In short, we can never know if the precept has been effectively applied. Accordingly, the most judicious application of Smith's precepts will not banish the problem of non-falsifiability. There is no way of showing that a close correlation between utility and experimental reward has been achieved. The idea that Smith's precepts 'work' is a classic article of faith, placed so far under surprisingly little methodological scrutiny.¹²

The limits of universality

Accordingly, a problem with the standard rationality assumptions is not that they lack empirical correlation, but that they could cover every conceivable decision situation and every possible causal mechanism underlying choice. Insofar as there may be common features of every decision situation then it may be possible to extract universal and meaningful propositions. Nevertheless, some important and specific features or causal mechanisms may be excluded by concentrating solely on the common features of every decision situation. In fact, the degree of universality involved is so great that it goes beyond the parameters of mere human decision.

¹² For a critical methodological discussion of Smith's precepts see Siakantaris (2000).

Recent theoretical and experimental studies confirm this high degree of universality, beyond the confines of human society. Experimental work with rats and other animals (Kagel *et al*, 1981, 1995) has ‘revealed’ that animals have downward-sloping demand curves, supposedly just like humans. Gary Becker (1991, p. 307) has argued extensively that: ‘Economic analysis is a powerful tool not only in understanding human behavior but also in understanding the behavior of other species.’ Similarly, Gordon Tullock (1994) has claimed that organisms – from bacteria to bears – can be treated as if they have the same general type of preference function that is attributed to humans in the microeconomics textbooks. They are all regarded as utility-maximisers. Accordingly, core concepts are not only applied to all forms of human society since the origin of our species, but also to a large portion of the animal kingdom as well. Seemingly, we now have ‘evidence’ of the ‘rationality’ of everything in evolution from the amoeba onwards. This suggests that such assumptions are telling us very little about specifically human societies, least of all about the unique complexities of modern human civilisation.

For the neoclassical economist, the fact that utility theory can ‘explain’ a wide variety of types of economic behaviour is regarded as a strong vindication of this general approach. I take a different view. First, the sheer generality of a theory tells us nothing of its explanatory power. We could conceive different general theories, such as that we all are programmed by aliens from outer space, or that we are all pawns of God. These would be quite general in their scope and could be applied in principle to any behavioural manifestation. But we would rightly be sceptical of their explanatory value. A theory does not explain anything unless it points to an underlying causal mechanism. In the case of individual behaviour, explanations must thus relate to the known mechanisms of the human psyche and human interaction and draw upon psychology, anthropology, sociology and other disciplines. This is precisely what the neoclassical advocates of utility theory refuse to do. They take the utility functions as given and give the job of grounding them theoretically to somebody else. By this refusal they indicate that utility theory itself cannot provide a real explanation.

Arguably, human societies are partly differentiated from other animals in terms of developed institutions and cultures. If utility maximising behaviour not confined to humanity, then these differentiating elements are effectively absent from the universal picture. Whether true or false, this picture can tell us little of importance about historically specific human cultures or institutions. That is the unintended achievement of the exponents of ubiquitous rationality and economic imperialism. The causal mechanisms through which culture and institutions mould and constrain human agents remain unexplored in this paradigm. Essentially, there is no adequate and substantial theory of human agency at the core of the standard theory. It tells us nothing of significance that is specifically about the human psyche or about human interaction. Outside the realm of the universal, no particular causal mechanism is identified by the theory. With respect to specifically human characteristics and specific human societies, it is causally vacuous. Its very weakness stems from its excessive universality. Indeed, to attain the status of universality it has to be evacuated of much of its real content.

This conclusion is consistent with Ernst Nagel’s (1961, p. 575) ‘principle of the inverse variation of extension with intension’. Although Nagel attempted to give further reasons for favouring general theories, his argument also suggested that universality is gained at the cost

of an ability to discriminate between and explain concrete particulars. This argument has some force against the claimed universality of neoclassical economics.¹³

Weak criticism and false approval

However, many critics of mainstream economics have taken a different line of attack. In a classic critique, Terence Hutchison (1938, p. 27) argued that the basic postulates of ‘pure theory’ necessarily suffered from a ‘complete lack of empirical content’. Many similar remarks have been made by heterodox economists, before and since. For example, the Post Keynesian economist Alfred Eichner (1983, p. 211), complained that the core assumptions of mainstream economic theory ‘have yet to be empirically validated’ and that they have ‘no empirical counterpart in the observable world’. However, the problem with these assumptions is *not* primarily their lack of empirical corroboration. It is that they are vessels into which *any* empirical content can be filled. The problem with the theory is *not* that it lacks empirical validation but that *any* conceivable fact about behaviour, from church attendance to suicide, can be fitted into the theory.¹⁴

Just as the critics of neoclassical theory wrongly claim that its basic postulates have been falsified, its exponents misleadingly claim that they have been rigorously confirmed. Jack Hirshleifer (1985, p. 59) went so far as to write: ‘Ultimately we must be ready to abandon the rationality paradigm to the extent that it fails to fit the evidence about human behavior.’ However, this apparent concession to empirical confirmation in fact conceals a methodological misunderstanding. Hirshleifer did not have to worry, because no conceivable evidence can ‘fail to fit’ some tortured version of the theory. Both Hirshleifer and the critics of the rationality paradigm share the flawed supposition: that evidence can in principle refute the theory. Both supporters and critics of neoclassical theory have perpetuated the myth that it is susceptible to decisive empirical testing.

As a result, the mainstream theory is not wrong because it is empirically inaccurate. It is not unrealistic in the sense that it fails to fit the data. Any data can be fitted into it. Hence no data can refute the theory. It cannot be displaced simply by an appeal to the evidence. The experimental evidence of preference reversals and other choice ‘anomalies’ may lead us to search for a different and better theory, but it does not in principle refute the old version based on utility and rational choice.¹⁵

Critics such as Hutchison (1938) and Eichner (1983) based their criticism on an untenable and empiricist view of science that denies that some non-falsifiable and ‘metaphysical’ assumptions are *essential to any science*. In fact, all sciences depend upon some propositions that are untestable. No theory can be composed entirely of empirically validated elements. Prior concepts are required to make sense of any fact. These prior concepts cannot all be

¹³ See Udéhn (1992). However, while I concur with much of Udéhn’s argument, he credits neoclassical economics with some relative success in explaining market, as opposed to non-market, phenomena. While the genuine achievements of neoclassical economics should not be denied, it concedes too much to suggest that it has an adequate definition and analysis of market institutions.

¹⁴ This is no joke. See Azzi and Ehrenberg (1975) and Hammermesh and Soss (1974).

¹⁵ I am not arguing that evidence is unimportant. Although evidence cannot falsify the theory, the accumulated evidence may provide a context in which the theory is more readily questioned. See Loomes (1998, pp. 485-6).

‘tested’ empirically. In any case, any ‘test’ itself relies on prior concepts or categories. As a result, all sciences must unavoidably make extensive use of some untestable and metaphysical assumptions.

Immanuel Kant (1929, p. 7) revealed in 1781 that human reason ‘begins with principles which it has no option save to employ’ but which ‘are no longer subject to any empirical test’. Accordingly, he recognised a role for metaphysics. Subsequently, in the heyday of positivism, the idea that metaphysics had any place in science was challenged. But from the 1950s, positivism itself was subjected to strong philosophical attacks. In particular, Willard van Orman Quine (1951) successfully overturned the view that all scientific and meaningful statements had to be based upon empirical experience. Eventually, Karl Popper also recognised that some metaphysical propositions are essential to science (Ackerman, 1976, pp. 30-1). The indispensable role of untestable and metaphysical assumptions is now widely accepted by philosophers.¹⁶

For this reason, the Hutchison-Eichner empiricist criticism of mainstream economics is untenable. In practice, furthermore, their denial of the essential role of non-falsifiable assumptions in any theory would disable any of their own attempts at theoretical construction. Given that it is practically impossible to test all assumptions, any theoretical construction would reveal hidden, ‘ad hoc’ assumptions, privileged to lie beyond empirical test. For reasons outlined above, every theory must involve some untestable assumptions. Hence any theory built on the claim of complete testability would be highly vulnerable to critique by its own canon.

However, this does not mean that ‘anything goes’ and that all criticisms are disabled. There are powerful theoretical criticisms of the rationality assumption. Essentially, the theory lacks adequate theoretical concepts to discriminate, understand and properly explain key phenomena. A problem with the standard assumptions of rationality and expected utility maximisation is their lack of specific theoretical and conceptual content, pertaining to specific causal mechanisms involved in the human psyche and in the structures of specific real world economic institutions.

To repeat: the empirical evidence is valuable and important, but it cannot be used to show that the theory is false. In recent years, there have been attempts to apply models of rational, utility maximising behaviour to a wide variety of phenomena, even beyond the sphere of commerce and markets. Models of utility-maximising behaviour have been applied to politics, marriage, religion, suicide, and much else. Such attempts have been widely resisted. Many tried to defend their academic discipline or subdiscipline from the ‘economic imperialism’ of rational choice models. However, the widespread failure to recognise the non-falsifiability of ‘rational’ maximising behaviour has weakened many such counter-arguments. They appealed to evidence: it was mistakenly argued that rational choice models did not fit the facts. On the contrary, models of utility-maximising behaviour can always be adjusted to fit the facts. The attempt to resist the incursions of rational choice theory by claiming otherwise was bound to fail. In this instance, appeals to evidence cannot win.

In development economics, for example, there was a debate in the 1970s over whether peasants were or were not ‘rational’. Critics of this idea appealed to ‘evidence’ of ‘non-rational’ behaviour, without realising that no evidence can strictly falsify the theory. With

¹⁶ See, for example, Caldwell’s (1982) critical discussion of positivism and Blaug’s (1992) account of the role of Lakatosian ‘hard core’ assumptions.

opponents weakened by their own theoretical position and methodological misunderstandings, the rational choice theorists seemed to win the argument (Popkin, 1979). Similarly weak defences were evident in sociology and political science, as they too were invaded by rational choice theorists. Again and again an attempt was made to resist the incursions of utility and rational choice, on the grounds that its assumptions are not 'realistic'. Such attempted defences against the invasion of rational choice theory are methodologically flawed and ultimately doomed.

The moral here is that mistaken claims concerning the testability of rational choice theory led its opponents to attack it with weak arguments. It would have been much more fruitful if both sides had admitted that the theory was falsifiable and then debated its explanatory value in specific circumstances. Instead, these controversies were entirely confined to claims and counter claims concerning empirical validation. At that primitive level the issue is simple: the assumptions of utility theory cannot be falsified.

The deconstruction of rationality

However, having almost conquered the social sciences, some of the rational choice theorists have become bored with their own weapons of victory. Ironically, it is beginning to be possible, even fashionable, for mainstream economists to question some of these core assumptions. Perhaps because mainstream economists have lost the capacity to police their own disciplinary boundaries, in search of a new separate identity they have begun to question their own *raison d'être*. As Kyriakos Kontopoulos (1993, p. 90) has pointed out: 'Ironically, economists become less economic at a time when sociologists seem to become enamored with rational choice theory.' Accordingly, some economists are now deconstructing rational economic man. As economist Robert Sugden (1991, p. 783) put it:

There was a time, not long ago, when the foundations of rational-choice theory appeared firm, and when the job of the economic theorist seemed to be one of drawing out the often complex implications of a fairly simple and uncontroversial system of axioms. But it is increasingly becoming clear that these foundations are less secure than we thought, and that they need to be examined and perhaps rebuilt.

One reason for this change of heart is the rise of game theory. In certain types of game the very definition of rationality becomes problematic. Nevertheless, the response of mainstream economists to these problems has largely to become immersed in the technicalities, rather than to give the economic agents at the core of the theory of human behaviour some real institutional and cultural flesh and blood. Some still cling tenaciously to the principles of rationality, in a manner that is reminiscent of Ptolemaic astronomers, fitting the evidence of the apparent circular movements of the stars into complicated models (Koestler, 1959). Others are not inclined simply to 'save appearances'; they express their misgivings but seem unaware where to look to find an alternative paradigm.

For some, the move to game theory has led to the questioning of core assumptions. For others it has reinforced the idea that economics itself is a formal game, with little connection to reality. If a theory makes no claim outside a single domain, then there is no aim to use the theory to explain other real world phenomena. The interest in the theory is typically in its mathematical content, rather than its usefulness to help understand reality. Accordingly, there is a move away from former attempts to build a universal theory (which turned out to be unfalsifiable), to the building of exemplifying theories that are not designed to be put under any empirical scrutiny whatsoever. There is a move from universal to 'what if?' theories. Step by step, mainstream economics is becoming disengaged with the real world. Instead of

looking at real institutional structures and mechanisms, it has become more and more involved in the niceties of mathematical technique.

Conclusion: the new frontier

Becker (1966) and others have attempted to show that factors such as ‘culture’ can be brought into rational choice theory by adding many arguments to the utility function. Among these arguments are cultural variables, so that the individual may seem to change his or her choices as a result of cultural shifts. The thrust of this type of analysis is to show that a more sophisticated utility function can fit behaviour where individuals react to cultural circumstances.

Again, the problem with this analysis is not that it fails to fit the evidence. The problem is that it is untenable on theoretical grounds. Becker’s approach seems to presume that each individual is born with a preference function that is already primed to deal with cultural circumstances of which the infant is ostensibly unaware. According to Becker, an individual’s preference function is already primed to deal with all sorts of foreign foods and ways, so that if we so happened to immerse ourselves in a foreign culture we would learn to like such things as sushi or sauerkraut. How could this be? Although we can imagine in the world of theory such a universal, culturally-sensitive utility function, the idea that it exists in our minds is implausible.

If, on the other hand, it is argued that this analytical approach does not assume that individuals *really* have such an utility function, but that it involves the assumption that people act *as if* they had one. The response to this argument is that, given the non-falsifiability of the rational choice approach, it is no big deal to show that people act *as if* they are maximising utility. Any behaviour can in principle be made consistent with a utility function. Where claims of specific predictions are involved, they are achieved by adding auxiliary, restrictive assumptions to the utility model (Shaper, 2000).

Take another important and related example. Learning is treated inadequately in the neoclassical economics (Hodgson, 1999). Basing itself on the idea of ‘rational economic man’, neoclassical economics has thereby to assume that the individual is capable of appraising all the known choice possibilities. Furthermore, each choice is assessed on the basis of a fixed ‘preference function’ which is mysteriously bestowed upon the individual at the beginning of its (adult?) life. Typically, neoclassical economics treats learning as the cumulative discovery of pre-existing ‘blueprint’ information, as stimulus and response, or as the Bayesian updating of subjective probability estimates in the light of incoming data. With the ‘input’ of this new information we are supposed, on the basis of our unchanging preference function, to determine mechanically our choices.

Again, we can ‘model’ learning by assuming complicated utility functions with many arguments. In the work of Becker (1996), the function is already ‘there’, ready to deal with unpredictable and unknowable circumstances. For instance, it already ‘knows’ how to react to the technology and inventions of the next century. Miraculously, its parameter space already includes variables representing the ideas and commodities of the future. Mysteriously, it has already learned how to recognise them. The question is posed as to what is meant by learning in such circumstances when we already know essentially what is to be learned? Such a conception of learning must be sorely inadequate.

In reality, instead of the mere input of ‘facts’, learning is a developmental and reconstitutive process. Learning is much more than a process of blueprint discovery, stimulus-response, input enhancement or statistical correction. Learning is a process of problem-

formulation and problem-solving, rather than the acquisition and accumulation of given 'bits' of information 'out there'. Learning is not the cumulative addition of knowledge upon a *tabula rasa*: it entails getting rid of old ideas as well as acquiring new ones. Developing the capacity to unlearn, and learn anew, is itself a part of the learning process. This process involves conjecture and error, in which mistakes become opportunities to learn rather than mere random perturbations. Neoclassical economics has fundamental problems with learning because the very notion of 'rational learning' is problematic. Learning involves adaptation to changing circumstances, in contrast to the neoclassical emphasis on equilibrium.

But note that the above is a theoretical criticism, rather than one based on falsification by evidence. The examples of culture and learning do not show that these phenomena cannot be fitted into some rational choice model. In principle, this can be done. The real question is: what is achieved and explained by such an approach? Furthermore, how does this approach explain the origin and development of the capacities that are encapsulated in the presumed utility function? If we are to take human evolution seriously, then it is implausible to suggest that humans can be understood in terms of utility functions that have arguments relating to future items or events (Hodgson, 1998).

It has been argued elsewhere (Hodgson, 2001; Potts, 2000) that adequate social and economic theory involves a focus on specificities as well as generalities. The trouble about general theories, even if they are valid, is that they cannot tell us very much about special cases or particular circumstances. At best, a general theory can account for variation in terms of different parameter values. It cannot readily accommodate the unique structural features of any particular phenomenon.

This is the problem with rational choice theory. It is over-general to the point of unfalsifiability. In response, it is a weak strategy of criticism to attempt to find instances where rational choice does not work. In principle a model can always be found to fit the phenomena. Accordingly, it is also a weak strategy of defence to attempt to show that a specific phenomenon fits into a rational choice mode. In principle this always can be done. The debate has to go beyond the weak arguments on both sides.

It can be freely admitted that, in a limited sense, people do make deliberate and 'rational' choices. If we notice two similar items of a different cost, then we may be rationally inclined to purchase the cheaper one. Such a rational calculus applies more readily to a world where information concerning quality and price are available and transparent, and cost-minimisation dominates our concerns. As Max Weber (1975) argued in 1908, rationality finds its fullest development in the pecuniary culture of modern capitalism. But this does not mean that the concept of rationality is adequate to describe all behaviour in the modern world.

Rational choice theory has invaded social sciences such as sociology with its devotees declaring triumphantly that it can 'explain' nuanced social phenomena such as altruism, honour, trust and duty. The point argued here is that it is ineffective to counter these allegations with denials of their *empirical* validity. This is a weak response because, in principle, any manifest behaviour can be fitted into the rational choice framework. However, once the unfalsifiable nature of rational choice theory is understood, we can meet the triumphant claims with the polite response: 'So what. We know *in advance* that any behaviour can fit the theory.' We are then able to move on to the more important question, concerning claims of explanation and their derivation. Explanation, we insist, is much more than empirical correlation. And a perfect empirical fit does not necessarily imply any explanation of the causal processes underlying behaviour. The debate then moves on to the vital question as to how such causal explanations can be obtained.

It is my personal view that economic and social theory can only advance if it takes account of historical, cultural and institutional specificities. The crusade for rational choice theory in modern social science is part of a wider project to develop a universal theory of all social phenomena. The problem with such a theory is that, in its excessive quest for generality, it will fail to focus on the historically and geographically specific features of the socio-economic systems that we wish to study and understand. As long as social theory is confined to generalities it will remain highly limited in dealing with any specific socio-economic system, including the one in which we live. What is required is a theory that is far more sensitive in this regard.

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