

economics & reality



Tony Lawson

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PREFACE AND ACKNOWLEDGEMENTS

'No reality, please. We're economists!' This heading to a couple of commentaries in a recent edition of *The Times Higher Education Supplement* (25 March 1994) captures an increasingly widely held perception. According to it, modern economics is largely irrelevant to an understanding of real world matters. Needless to say this view is not accepted by all economists. And it does seem *prima facie* implausible as a characterisation of an academic discipline that is often represented as a social *science*. Yet, my assessment set out below is that it is not wholly mistaken; there *is* a sense in which social reality is neglected in much of modern economics. I shall indicate furthermore how this neglect is debilitating of the subject, with unfortunate repercussions for the multitude of further activities that draw upon it.

My purpose here, however, is not merely to identify limitations of the discipline but also to work towards transcending them. My overall aim, if somewhat unfashionably, is to bring reality (or more of it) back into economics. Of course, in formulating my goal in this way, and in acknowledging some validity to the perception of the discipline noted above, I am not wanting to suggest that modern economics is devoid of a conception of reality altogether. Indeed, it could not be. For the adoption of any method, practice or goal, presupposes a world-view, even if it is left implicit and unexamined. The feature of modern economics which is remarkable here is the minimal concern that is shown either for tailoring methods to insights available regarding the nature of the social world, or for explicitly determining the sorts of conditions under which chosen methods would be appropriate. This is despite a dearth of successes according to criteria which the discipline sets itself. Rather, methods and procedures are formulated according to the nature or degree of their technical sophistication, or their conformity with *a priori* conceptions of proper practice, or some such. The majority of economists seem merely to take it for granted that if ever real world problems or situations are to be studied the chosen methods will necessarily 'fit'. It is in this sense that reality is neglected in modern economics. And we will see that it is just

the presumption that methods of economic analysis can be fashioned without explicit regard to the nature of social phenomena that accounts for the subject's numerous failings.

In short, contemporary economics is marked by an effective neglect of *ontology*, by a lack of attention to elaborating the nature of (social) being or existence. And the project reported here is motivated by a desire to compensate for this neglect. The objective is to determine a sustainable account of natural and social being with a view both to explaining and resolving numerous problems which beset the discipline and to gaining an informed vantage-point on the sort of practices that are likely to bear fruit. Theories which address these sorts of concerns and involve commitments to the reality of features elaborated are usually collected under the heading of philosophical (or ontological) *realism*. The position sustained in this book, then, is an explicitly realist one. The book constitutes a realist theory of and for economics.

Its context can be briefly sketched. In contrast to much contemporary work on economic methodology, the project reported here did not emerge from a formal study of philosophy. Having come to economics by way of first studying mathematics I was immediately impressed by, as I saw it, the widespread and rather uncritical application of formalistic methods and systems to conditions for which they were obviously quite unsuited. In consequence, my interests turned fairly quickly to questions of ontology, and specifically to the study of how methods and modes of reasoning might be fashioned to insights concerning the nature of social being. However, I first approached these issues by way of reading economists rather than philosophers. Given my immediate conviction that the conventional economic methods and procedures are rather obviously inappropriate to the successful investigation of social reality, I assumed that many other economists would have reached the same conclusion long before. I thus supposed they would already have investigated and responded to the situation. In any case, it was an understanding of the economics context specifically that was of primary interest to me.

It turned out that it is mostly (but not exclusively) economists regarded as non-mainstream who have reflected on these matters to any significant extent. I refer here to the likes of Keynes, Hayek, Marx, Dobb, Veblen, Marshall, Smith, Shackle, Menger, Boulding, and Kaldor. In consequence, it is their writings that provided much of the initial background material for the central argument set out below.

In fact, they contribute rather more than this. In one form or another, they already express many of the fundamental tenets of the basic thesis. For example, when Keynes writes that:

If we were dealing with ... independent atomic factors and between

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them completely comprehensive, acting with fluctuating relative strength on material constant and homogeneous through time, we might be able to use the method of multiple correlation with some confidence for disentangling the laws of their action.

(Keynes, 1973b: 286)

and Marx observes that individuals:

make their own history, but they do not make it just as they please; they do not make it under circumstances chosen by themselves, but under circumstances directly encountered, given, and transmitted from the past.

(Marx, 1977: 300)

and Veblen suggests:

The growth and mutations of the institutional fabric are an outcome of the conduct of the individual members of the group, since it is out of the experience of the individuals, through the habituation of individuals, that institutions arise; and it is in this same experience that these same institutions act to direct and define the aims and end of conduct.

(Veblen, 1909: 243)

and Hayek adds that:

different rules apply to different individuals according to age, sex, status, or some particular state in which each individual finds itself at the moment.

(Hayek, 1967: 68)

each is expressing in condensed form an important aspect of the thesis which follows. But what a lot of fleshing out and unpacking each assessment requires.

When I eventually turned to formal philosophy, I naturally sought out the writings of philosophers who have been especially concerned with ontological or metaphysical questions. The influence of this literature on what follows appears to distinguish further my project from most other contributions in economic methodology. For although the latter contributions tend to be heavily rooted in the philosophy of science literature, it is questions of epistemic appraisal (i.e. epistemological questions concerning the rational basis for accepting or rejecting theories) that have dominated the economic methodology discussion. This discussion draws primarily upon the philosophies of Popper, and of others like Lakatos who responded to Popper's work (see contributions to Backhouse, 1994, and for a useful overview see Boylan and O'Gorman, 1995). Mäki (1993a: 5) sums up this situation by suggesting that

'A Popperian dominance, a kind of Popperian mainstream in economic methodology has prevailed'. It is not clear to me that this emphasis upon epistemological matters, often more or less detached from explicit ontological theorising, has been overly fruitful. Indeed, I feel inclined to support Mäki's assessment 'that the Popperian dominance has led to a misallocation of intellectual resources in economic methodology' resulting in a neglect of, amongst other things, a questioning of 'the nature of metaphysical commitments involved in actual research practice' (1993a: 6). This book, then, can also be seen as an attempt to redirect the methodological debate in economics.

Even so, any philosophical position, including an ontologically oriented one such as defended below, ultimately bears epistemological implications. In consequence, some readers may still feel some surprise to learn that Popper and/or Popperian (epistemological) themes and categories figure relatively little – significantly less than in comparable texts concerned with economic methodology. This 'omission', moreover, may appear especially surprising in the light of the fact that Popper openly proclaims himself a realist, fallibilist, and anti-positivist, and recognises the possibility of demarcating science from non-science – where all these are positions openly accepted in what follows.

The basic reason for it is that I not only adopt a more explicit focus upon ontological issues but also defend a theory of ontology which is very different to that presupposed by Popper, at least in his most familiar writings on falsificationism (as opposed to his relatively recent (1990) rather general remarks upon propensities). In consequence, the epistemological questions and implications are correspondingly different. In parallel with Popper I am certainly concerned to criticise various results of the (untenable) philosophical perspective of positivism. But although Popper (1992) claims responsibility for 'killing' positivism, or at least the *logical positivism* of the Vienna circle, there never was a single fatal blow. Positivism, including logical positivism, rests upon at least two fundamental assumptions. Not only does it suppose that science is essentially *monistic* in its development, it also assumes that it is *deductive* in structure. If Popper's contributions to the theory of knowledge, and in particular his work on falsificationism or 'deductivism' (Popper, 1992: 81), have helped undermine the former aspect, these same writings (at least as most have interpreted them) leave the assumed deductivist structure untouched. And it is the presumption of a deductivist structure of science, including its implicit account of *being*, or ontology, that I am concerned here to criticise before anything else.

I should immediately add, however, that if the falsificationist or deductivist programme usually associated with Popper is rejected here as a generalised way of proceeding, Popper's explicit commitment, in that programme, to the pursuit of objective truth and rationality in

science is endorsed. Indeed, at a time when so many economists appear to do little more than flit from fashion to fashion; when, with one or two notable exceptions (e.g. Mäki, 1988, 1992a, 1992b, 1994), economists rarely even mention (let alone accept) the goal of pursuing truth; and when, despite everything, many mainstream economists persist in claiming the authority of Popper for what they do; it seems worthwhile restating Popper's views on these matters explicitly:

I guess, indeed, that it is the suppressed sense of our own fallibility that is responsible for our despicable tendency to form cliques and to go along with whatever seems to be fashionable: that makes so many of us howl with the wolves. All this is human weakness, which means it ought not to exist. But it does exist, of course; it is even to be found among some scientists. And as it exists, we ought to combat it; first in ourselves, and then, perhaps, in others. For I hold that science *ought* to strive for objective truth, for truth that depends only on the facts; on truth that is above human authority and above arbitration, and certainly above scientific fashions. Some sociologists fail to understand that this objectivity is a possibility towards which science (and therefore scientists) should aim. Yet science *has* aimed at truth for at least 2,500 years.

(Popper, 1990: 34)

There are a number of philosophers (Harré, Hesse and Toulmin) who have focused their criticism on the supposition that science is deductive in structure, and whose writings have had an influence on what follows, just as there are philosophers other than Popper (Bachelard, Lakatos, Kuhn and Feyerabend) who have attacked the positivistic view of scientific development as monistic. But in my view it is Bhaskar (1978, 1979, 1986, 1989) who has managed most successfully to combine the insights of *both* tendencies in constructing a sustainable ontology for science, and whose project has in consequence proved most useful for my own. Although I only came across it in the late 1980s it has had a very significant impact upon what follows.

Nevertheless, I should stress that even Bhaskar's work is not explicitly surveyed here. Influential though Bhaskar's writing has been, the present book does not constitute an introduction to it, or anything like it; for that the reader can consult not only Bhaskar's own contributions but also Andrew Collier's (1994) useful introduction and appraisal. In structure and content the current book is shaped by the situation in economics. As Bhaskar somewhere explicitly acknowledges, his ideas and terminology are not consistent in the sense of being fixed over time. Rather they reveal a developmental consistency: as new problems are resolved, wider concerns are taken on board and earlier conclusions and terminology are revised accordingly. In intent, at least, any consistency to

be found below with Bhaskar's contribution is also of this developmental sort.

This book, then, is about economics, its current state and real possibilities and limits. Although the thesis which follows may be interpreted as broadly philosophical, it takes its premises from, and so is firmly rooted in, problems, practices and writings of economists. The intended readership is, above all, economists, especially those prepared at least to question received doctrines. The central argument which follows does have a wider relevance, but the book was written with the economics community in mind. It is the product of a continuing dissatisfaction with the dominant orientation of the economics discipline combined with a conviction that it is possible to do significantly better.

A brief overview of the argument can be provided by summarising each part of the book.

Part I In Chapter 1 I initiate the discussion through drawing attention to various problems and tensions which pervade economics. In Chapter 2 the widespread acceptance of a particular 'deductivist' conception of science and explanation ultimately rooted in, or at least parasitic upon, positivist results is indicated, and an alternative 'transcendental realist' conception is outlined. The relative superiority of the latter over the former is demonstrated in Chapter 3. In Chapter 4 the problems and tensions of contemporary economics which are noted and described in Chapter 1 are shown to derive from the widespread reliance upon the deductivist conception of science and explanation, while the superior transcendental realist account is found to be free of such problems. In Chapter 5 the reasoning used to establish these results is examined in some detail. Further implications of the perspective supported are elaborated in Chapter 6.

Part II With the central argument broadly laid out in Part I, the object of Part II is to examine in greater detail the various ways in which mainstream economics turns on the deductivist conception of science and explanation and to indicate the sorts of problems that arise in each case. The focus is upon econometrics in Chapter 7; upon 'economic theory' in Chapter 8; and, in Chapter 9, upon those variations on the latter which attempt to play down the need to explain (yet) any actual phenomena. In Chapter 10 I show that the lingering effects of positivism even shape Hayek's early, and currently influential, attempt to develop a 'subjectivist' alternative to the mainstream approach. The essential characteristics of the various projects considered in Chapters 7 to 10 are summarised in Chapter 11, along with conceivable alternatives which also fail to abandon positivist restrictions.

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Part III A basic position sustained in Part I is that there are benefits to continually modifying methods of analysis in the light of insights gained into the nature of any given object(s) of enquiry. With this in mind, the focus of Part III is upon social being, and specifically upon the nature of human agency, social structure and their irreducible mutual interdependency. While Chapter 12 is concerned with social structure Chapter 13 concentrates on human agency, including aspects of subjectivity and the nature of human rationality.

Part IV With a conception of social being set out in Parts I and III, the various consequences for economic scientific practice are elaborated in Part IV. In Chapter 14 I draw out implications concerning the basic orientation of economic explanation, and in Chapter 15 I discuss how competing explanatory theories of social phenomena can be assessed. In Chapter 16 I focus upon the much neglected yet fundamental issue of scientific abstraction. The epistemic status of explanatory results is considered in Chapter 17 and the possibility of objective truth is assessed. Chapter 18 constitutes an illustrative example of the explanatory approach defended.

Part V The final part of the book rounds off the argument. In Chapter 19 I trace out the implications for questions of policy and intended change and in Chapter 20 I draw together the various threads of the argument by way of examining the consequences for the enterprise of economic prediction. The basic overall conclusion is that while the academic discipline stands in need of radical transformation, involving amongst other things the abandonment of many of its most cherished ways of proceeding, the possibility of realising an efficacious and emancipatory science of economics is far from being out of the question. Such an achievement, though, will inevitably involve a greater sensitivity to context and a rather more reflective and critical orientation than are currently in evidence.

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The final task that awaits me at this point is the pleasant one of acknowledging at least some of those who have contributed to the preparation of this book over its numerous stages. First of all I must thank those who have helped me to type it. I am ashamed to admit that it was only very recently that I learnt to use a 'word processor'. Thus although I actually managed to type recent versions myself, the project has been under way for sufficiently long that various people have typed early drafts of chapters. In particular I thank Louise Cross, Nacirah

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