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EMPIRICAL REALISM AS META-METHOD

Tony Lawson on neoclassical economics

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Introduction

Tony Lawson has, over the last few years, established a reputation as the economics profession's staunchest and most prolific defender of Roy Bhaskar's transcendental realist philosophy of science. In a series of papers beginning in the late 1980s Lawson systematically defended Bhaskarian realism as a general philosophical framework for understanding scientific knowledge – both natural and social – while at the same time using this philosophical perspective as the springboard for a sustained critique of the theoretical and empirical practice of mainstream economists.[1]

The breadth of Lawson's realist programme provides his critics with a potentially wide range of possible targets for critical examination. Detractors might contest: realism in general; Bhaskar's transcendental realism in particular; Lawson's own reading of realism, Bhaskar, or any other aspect of his philosophical discussion; the Lawson/Bhaskar reading of the primary alternative to transcendental realism (called empirical realism); or even the particular application of transcendental realism to the social sciences (called critical realism).[2] While the following discussion will provide a critical commentary on Lawson's project, it will not focus on any of these philosophical issues; in fact, there will not be any attempt to criticise Lawson's (or Bhaskar's) realist position at all. My decision not to challenge any of these core philosophical notions should not be interpreted as a statement that such issues are uninteresting, or not legitimate fodder for critical discussion, or that Lawson's Bhaskarian philosophy is beyond reproach. It simply means that this is not the place or the time for debating philosophical realism; the critical focus in this chapter will be on economics and economic methodology.

The following discussion will focus exclusively on *Lawson's reading of neoclassical economics*, or to be more specific, *his argument that the philosophical underpinning of modern neoclassical (mainstream) economics is empirical realism*. Lawson's reading will be challenged directly – by looking at the type of theoretical activity that actually occurs within mainstream economics – and also by examining what various philosophers and economic methodologists have said about the defining characteristics of neoclassical theory. The bottom line of this discussion will be that Lawson's claims are way off base – that neoclassical economics is not in any sense the empirical realist-inspired inquiry that Lawson makes it out to be – in fact, it seems to be more consistent with the type of transcendental realism that he himself endorses.

The chapter is arranged as follows. The first section briefly reviews transcendental realism, empirical realism, and Lawson's reading of mainstream economics. The next section briefly examines the theoretical practice of economists and demonstrates that while it may not be clear exactly what mainstream neoclassical theory is, it most clearly is not an exercise in empirical realism. The following section briefly examines the history of methodological discourse in economics and demonstrates that the vast majority of commentators have not found economics to be consistent with empirical realism (and that is often seen as a problem) and that it is actually closer to transcendental realism (and that, for many, is also a problem). The conclusion offers some comments about the implications (and irony) of Lawson's particular reading of modern economic theory.

Empirical realism and neoclassical economics

Roy Bhaskar's philosophy of science was presented in a series of books and articles over the last twenty or so years.[3] His general approach has been to uncover a number of fundamental tensions within the positivist-inspired view of scientific knowledge and to posit his own transcendental realism as an alternative (and solution) to these problems. Bhaskar is in many respects a very contemporary philosopher of science; by this I mean that he (like so many recent philosophers) tries to walk a middle ground between the traditional concerns of *philosophers* (as opposed to historians or sociologists) of science and the contemporary recognition of the social, and perhaps even socially constructed, nature of the scientific enterprise. While Bhaskar seems to be quite contemporary in his approach, he is not a philosopher that gets much attention from the general philosophical community – this is in spite of the fact that his arguments often have much in common with various other philosophers (Cartwright 1989, for example) who do get discussed in the philosophical literature. Bhaskar's main impact has been – and this is in part due to the work of individuals such as Tony Lawson – in the philosophy of social science.

Bhaskar's approach, unlike most philosophers of science, is *ontological*. He argues that any theory of knowledge, any epistemology, necessarily presupposes some (perhaps implicit) ontological commitment regarding the objects of that knowledge. Empiricist epistemology, the epistemology of mainstream philosophy of science, inspires an implicit ontology of *empirical realism*, which makes the objects of scientific investigation the same as the objects of sense experience. Since those things that can be observed, the objects of sense experience, are most often empirical event regularities, event regularities become the objects of (the only objects of) scientific inquiry. As Lawson characterises the situation, science becomes concerned exclusively with identifying regularities of the form 'whenever event (type) x then event (type) y'. Bhaskar refers to this view as the *epistemic fallacy*: the fallacy of reducing matters of ontology (existence or being) to matters of epistemology (knowledge).

This epistemic fallacy generates an *ontological tension* (Bhaskar 1989: 18) in at least two different ways. First, it generates a tension between the standard philosophical characterisation of scientific knowledge and the ontological presuppositions of practising scientists. Practising scientists actually look for the underlying, hidden, causal mechanisms that generate the empirical regularities they observe, and consider these underlying causes, not the empirical regularities themselves, to be the proper objects of scientific inquiry. A second, related, tension emerges within the experimental practice of science. Successful experimental practice always entails structuring the environment so that the effect of a single causal mechanism can be isolated; it requires the artificial structuring of the experimental context so as to eliminate or neutralise the impact of all other causal mechanisms other than the one under consideration. The empirical regularities that are supposed to be at the heart of science can only be observed in the closed and human-constructed environment of experimental systems; implying, of course, that there is nothing 'natural' about the domain of natural science.

The necessity of experimental closure doubly vindicates realism. On one hand, the 'facts' of science are clearly a social product; the observable facts of science 'are real; but they are historically specific social realities' (Bhaskar 1989: 61). This means that if, as empiricism suggests, the 'laws of nature' are factual regularities, then 'we are logically committed to the absurdities that scientists, in their experimental activity, cause and even change the laws of nature!' (ibid.: 15–16). On the other hand, in order to apply science outside the environment of the laboratory one must presuppose that the same causal mechanisms that were empirically revealed in the closed experimental context will continue to act in the more complex open environment outside the lab, again suggesting that something must be going on other than the constant conjunction of empirical events. All of this adds up to an extremely problematic situation for empiricist philosophy of science; Bhaskar offers transcendental realism as a solution to these problems.

Transcendental realism starts with an ontological distinction between the underlying causal laws (generative structures, capacities, causal powers, mechanisms, etc.) and the observable patterns of events (empirical regularities). These underlying causal mechanisms are the *intransitive* objects of scientific inquiry, while the empirical regularities are the *transitive* products of scientific investigation. These causal laws are tendencies which may or may not exhibit themselves empirically in any particular situation. In the complex and open world outside the experimental environment there are many causal forces at work, many *tendencies*, and that which becomes empirically manifest is co-produced by the interaction of these multiple causal factors. These empirical manifestations are more likely to be observed within the context of a closed experimental environment, but that is the purpose of the experimental set-up; the 'experimental activity can be explained as an attempt to intervene in order to *close* the system, in order, in other words, to insulate a particular mechanism of interest by holding off all other potentially counteracting mechanisms' (Lawson 1994a: 268, emphasis in original). The process of scientific development is the process of uncovering ever deeper layers of these causal forces; the intransitive domain of these causal forces exists independently of our scientific investigation, but the scientific investigation is itself a transitive and historically contingent social process. Transcendental realism simultaneously sustains the claims that: (1) the object of science is to uncover non-observable causal laws that exist independently of our theorising about them, and (2) that science is socially produced and its empirical domain does not exist independently of our theorising.

> Now I have argued . . . that constant conjunctions are not in general spontaneously available in nature but rather have to be worked for in the laboratories of science, so that causal laws and the other objects of experimental investigation must, if that activity is to be rendered intelligible, be regarded as ontologically independent of the patterns of events and the activities of human beings alike; and that, conversely, the concepts and descriptions under which we bring them must, if *inter alia* scientific development is to be possible, be seen as part of the irreducibly social process of science. Thus experiences (and the facts they ground), and the constant conjunctions of events that form the empirical grounds for causal laws, are social products. But the objects to which they afford us access, such as causal laws, exist and act quite independently of us.
>
> (Bhaskar 1989: 51)

As Lawson and others have emphasised, Bhaskar's transcendental realism has particularly strong implications for the social sciences. For one thing, social systems are inherently open, which makes it particularly difficult to find useful empirical regularities within the social context; an implication of this openness is that human science will be much more concerned with explanation than prediction. For another thing, the social nature of 'fact production' is even more significant in the social sciences where the (social) process of science is more clearly, and more inseparably, intertwined with the (social) object domain. For Bhaskar, the human sciences are clearly 'sciences', but they have their own unique characteristics and he does not in any way support their reduction to biology or physics.

> To sum up, then, society is not given in, but presupposed by, experience. But it is precisely its peculiar ontological status, its transcendentally real character, that makes it a possible object of knowledge for us. Such knowledge is non-natural but still scientific. As for the law-like statements of the social sciences, they designate tendencies operating at a single level of the social structure only. Because they are defined only for one relatively autonomous component of the social structure and because they act in systems that are always open, they designate tendencies (such as for the rates of profit on capitalist enterprises to be equalised) which may never be manifested. But they are nevertheless essential to the understanding and the changing of, just because they are really productive of, the different forms of social life.
>
> (Bhaskar 1989: 87)

Lawson wholeheartedly endorses Bhaskar's transcendental realist critique of empirical realist-based philosophy of science (termed 'positivism' by Lawson)[4] and also the application of his view to the discipline of economics (critical realism). But for Lawson critical realism is the way that economists *should* think about economics; it is *not the way that economists actually do think about their discipline.* According to Lawson, modern neoclassical economics is not concerned with identifying the underlying, intransitive, causal mechanisms that generate economic phenomena, or in characterising the tendency laws that are in operation in the background of economic life; instead it is driven by the search for the type of constant conjunctions and event regularities that characterise the positivist approach to scientific knowledge. Not only is empirical realism the philosophical vision that undergirds economic theorising, this empiricist vision is also, according to Lawson, the main reason for the discipline's many failures. Both the substantive claims and the heuristic practice of mainstream economics are a product of the positivist approach to scientific knowledge and its associated 'deductivist' approach to explanation.

> Now it is not merely the case that these two positivistic results or features – the event regularity conception of science and a social theory based upon the atomistic individual – are widely accepted in contemporary economics; they are, I suggest, definitive of it. Together, they determine both the structure of orthodox analysis as well as its material form. . . . For such reasons, if to repeat, I suggest that these two results, along with the positivistic perspective from which they derive, be recognized as essentially definitive of the orthodox project. I know of no other interpretation that can account for the sweep of orthodox analysis so readily.
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> (Lawson 1994c: 113)

Two things need to be emphasised about Lawson's reading of the relationship between empirical realism and economics. First, this is a very *strict version* of empiricism. It is the radical empiricism of David Hume, and not one of the much weaker forms of empiricism endorsed by the logical empiricists or the early Popper (or by contemporary philosophers of science). Second, Lawson is asserting much more than the familiar claim that orthodox economists pay lip-service to positivism; he is arguing that positivist ideas really do effect both the form and content of mainstream economic theory. This is not simply about positivist rhetoric in economics; it is a much stronger claim about the causal power of positivist ideas in determining the conduct of modern economic inquiry.

Regrettably the intensity of Lawson's commitment to this empirical realist reading of contemporary economics is not matched by either the quantity or the quality of the evidence that he garners in defence

of his reading. In fact, Lawson offers very little evidence to support his claim that positivism has an overarching impact on modern economics. He frequently makes statements such as 'I do not think it is contentious to observe that deductivism so understood characterizes contemporary economics' (1994a: 260) or that it is 'the misguided adherence to this conception of science, . . . including accepting the universal applicability of the deductivist form of explanation, that constitutes the fundamental problem in the economic scientific project' (1995: 18), but there is seldom any real defence of these statements. Lawson does not provide any serious case studies in the history of economic thought, or any detailed investigations of the theoretical practice of economists; in the end his argument amounts to little more than proof by repeated assertion.[5] Let us try to provide this missing link; let us consider neoclassical economics more carefully and see if empirical realism is as influential as Lawson claims in these repeated assertions.

What neoclassicism is not: economic practice

Recall that according to empirical realism only atomic sense experiences exist, and to explain something entails deducing it from a set of initial conditions and a universal law (event regularity) of the form 'whenever event (type) x then event (type) y'. In particular, according to the positivist view, science is not concerned with the identification of the underlying structures and causal mechanisms that govern the phenomena of experience or to explain this phenomena in terms of such structures or causal mechanisms. Is this what mainstream economists do, or attempt to do?

Since 'mainstream' (or 'orthodox' or 'neoclassical') economics could arguably entail a wide range of different theoretical and/or empirical activities, and it is not clear how far back into the history of economic thought one might go and still find 'mainstream' economics, let me simply examine the one case that Lawson himself considers: Walrasian general equilibrium theory circa 1970.[6] Lawson obviously considers this particular research programme at this particular time to be an example (perhaps the paradigm example) of what he means by mainstream economics. To be more specific let us consider the canonical text of the genre: Arrow and Hahn (1971).

Arrow and Hahn's first content chapter is Chapter 2. In this chapter the authors provide a simple existence proof for the general equilibrium price vector (p*) in a Walrasian economy characterised in terms of continuous aggregate excess demand functions. Their main concern is:

> the description of situations in which the desired actions of economic agents are all mutually compatible and can all be carried out simultaneously, and for which we can prove that for the various economies discussed, there exists a set of prices that will cause agents to make mutually compatible decisions.
>
> (Arrow and Hahn 1971: 16)

Where is the event regularity that this exercise is supposed to explain? Such existence proofs show that certain things are *possible* in certain hypothetical worlds (worlds that are admittedly far simpler than ours). Such proofs provide, as Daniel Hausman has argued, a type of 'theoretical reassurance'; such demonstrations of existence 'give one reason to believe, in Mill's words (1843, 6.3.1), that economists know the laws of the "greater causes" of economic phenomena' (Hausman 1992: 101). This is a transcendental realist role, not an empirical realist role, for such existence proofs. We want our economic theories to isolate the underlying 'greater causes' of the phenomena that we observe, and one way that we can obtain the 'reassurance' that we have in fact isolated these greater causes is to see if it is *possible* that such causes would be consistent with the type of coordinated activity that seems to prevail in a market economy. Existence proofs provide us with some reassurance that we are 'on the right track' (Hausman 1992: 101) in this essentially realist endeavour. Perhaps the closest that anyone has come to linking such proofs to something like event regularities was in Roy Weintraub's early work on the history of general equilibrium theory, and even in that case the linkage was indirect and very weak. Weintraub (1985) argued that existence proofs were part of the hard core of a Lakatosian research programme in general equilibrium theory and that the protective belt of that programme included many different applied theories that did have empirical implications. The fact that even this very weak empirical linkage was severely criticised (and abandoned by Weintraub in later work) only demonstrates how ineffective an empiricist vision is in explaining the primary theoretical activity of Walrasian economics (proving existence).

Other aspects of Arrow and Hahn's second chapter are equally inexplicable in event regularity terms. Even the aggregate excess demand functions themselves – something that might conceivably be observable – are not. These excess demand functions are:

> an *ex ante* concept; it is hypothetical in the sense that the actual purchases and sales may differ from those that the theory of the decisions of agents tells us would be the purchases and sales regarded as proper by the agents at p.
>
> (Arrow and Hahn 1971: 19)

In other words, even these excess demand functions are not in any sense empirical; they are hypothetical demands representing what (abstractly well-behaved) agents would want to buy if in fact they could sell what they wanted to sell (and buy the other things they wanted to buy) at the price vector p, but of course unless the price vector is the equilibrium price vector (unless $p = p^*$), that is, unless that which we are trying to prove the existence of has already come to pass, then these hypothetical demands will not be 'demands' at all (even in this pristine Walrasian world). Finally, it should be noted that these excess demand functions are assumed to satisfy both Walras' law (W) and zero degree homogeneity (H), two assumptions that are almost never found to hold on empirically estimated aggregate demand functions.[7] All of these things certainly suggest that whatever is driving the theoretical activity in Chapter 2 of Arrow and Hahn it is most certainly not the positivist-inspired search for event regularities, and if Hausman's argument is accepted, it looks much more like (at least one aspect of) a search for the actual causal mechanisms behind the phenomena of the competitive market.

Chapter 3 is about production. In this chapter single-output firms, with knowledge of all technically possible relationships between their output and all the various combinations of inputs that they could possibly have access to, engage in timeless ('inputs and outputs are contemporaneous' 1971: 53) production. It is not exactly clear what status such a firm might have in a positivist world where the only meaningful propositions are those involving sense experiences or the purely analytic propositions of logic and mathematics, but it certainly seems that the latter would be more likely than the former. These firms maximise a continuous profit function defined over a bounded and strictly convex production set that admits free disposal. These firms are shown to generate an aggregate supply correspondence that is continuous. We find ourselves at the end of Chapter 3 and we did not encounter a single event regularity, or even anything that might strictly be considered observable.

Arrow and Hahn's Chapter 4 is entitled 'Consumer Decisions and Efficient Allocations'. The 'households' in this chapter each have well-ordered preferences (transitive, continuous, non-satiated, etc.) which can be represented by a 'continuous, semi-strictly quasi-concave utility function' (1971: 87) – an assumption that seems to be radically at odds with the ostensibly constitutive mandate that only those things that are empirically observable can be considered (or even exist). The main result in the chapter is the demonstration that a competitive equilibrium for a pure exchange economy composed of such consumers is *Pareto efficient* (1971: 93). Pareto efficiency is of course a normative notion; it has to do with making one person 'better off' without making someone else 'worse off'. In a positivist world composed exclusively of sense experience and event regularities such notions are *simply meaningless nonsense* (literally non-sense). They are meaningless propositions derived from a hypothetical economy composed of agents whose only defining characteristic is a mathematically well-behaved but unobservable (and thus nonsense) utility function. There doesn't seem to be much empirical realism in Chapter 4 either.

Well, I could go on, but the point seems to be pretty clear – whatever is happening here it certainly does not appear to have any direct relationship to the positivist conception of scientific theorising. I should also add that things get even worse (if that is possible) in later chapters. Chapter 9 is about uniqueness – the restriction that there exists only one equilibrium price vector – how could uniqueness be observable? Non-uniqueness could perhaps be observable, but not uniqueness. Chapters 11 and 12 are about stability, that (clearly) transcendental realist concept of a 'tendency to equilibrium' (1971: 263). The final chapter is an attempt to analyse a Keynesian model with money, expectations, and the possibility that Walrasian (target) demands are not active in determining the course of economic activity; this chapter is best interpreted as another attempt to get at the real causal mechanisms at work in a market economy. It seems impossible to conceive of a theoretical construction that is less directly inspired by the Humean radical empiricist view of scientific knowledge than Walrasian general equilibrium theory. Perhaps other parts of 'mainstream' economics are slightly more inspired, but general equilibrium theory (particularly Hahn's version) is Lawson's most cited example.

What neoclassicism is not: methodology

Even a brief look at the literature on economic methodology would indicate that the lack of fit between Walrasian general equilibrium theory and the rather extreme version of empiricism that Lawson calls empirical realism is more the norm than the exception in economics.[8] The development of economic theory from Adam Smith to that which appears in the most recent theoretical journals has never been guided, much less constituted, by a pristinely Humean notion of what constitutes legitimate scientific knowledge. Of course this is not to suggest that 'the facts' do not matter at all, or to claim that economics is not empirical in some rough and tumble sense – and it is also not to deny the role, and persuasive power, of focused empiricist rhetoric in the history of economic thought (as both Samuelson's operationalism and Friedman's predictivism clearly demonstrate) – but it is to deny any significant causal role to the type of radical empiricism that Lawson calls empirical realism. Finally, this also does not assert that the history of economic thought is totally devoid of individuals who were guided by radical empiricism on some particular topic (Henry Ludwell Moore and Wesley Clair Mitchell come immediately to mind), only to assert that it has ever been the driving force behind most of what has appeared in mainstream economics. The history of methodological discourse in economics clearly bears this out.

Positivist ideas had their first serious impact on methodological discourse with the publication of Terence Hutchison's book in 1938. Hutchison was reacting specifically to Robbins (1932), but at that time Robbins simply represented the most recent version of the Millian tradition in economic methodology. From Hutchison (1938) through Blaug (1992) economists committed to empiricism have argued persuasively that while economists pay lip-service to empiricism (they preach it), they do not in fact behave according to its precepts (they do not practise it).[9] Now neither Hutchison nor Blaug subscribe to the type of radical empiricism that Lawson considers to be the guiding spirit of mainstream economics – both are Popperian falsificationists – but this is precisely the point. Even those methodologists who advocate a much weaker form of empiricism than that which Lawson attributes to mainstream economics have argued systematically and persuasively that empiricism has not played a very important role in the evolution of economic theory.[10] If the enervated empiricism of Hutchison and Blaug has not been constitutive of economic theorising, then how could Lawson's empirical realism play such a role?

The history of methodological discourse not only demonstrates that radical empiricism has played very little role in the development of economic thought, it also demonstrates that the Millian tendency law view that Lawson advocates has played a rather significant role. The argument that economics is a separate science that cannot – because of the type of mechanisms that govern economic phenomena – be conducted in strict compliance with the empiricist method of natural science, has been an influential perspective in the history of economic methodology. In fact, this is, much more than positivism, the *traditional* (and perhaps even the dominant) characterisation of the method of economic science (by both supporters and critics). Versions of this argument appear of course in Mill, but also in Cairnes, Neville Keynes, Robbins, and most recently in Hausman (1992). This is not to say that all of these authors endorse the exact same philosophical perspective (particularly Hausman), but they do all argue that a strictly Humean approach to economic science is impossible, that it is impossible because of the essential nature of (the underlying causal forces at work in) the subject matter of economics, and that some version of a tendency is the only reasonable way to think about 'laws' in economic science. Lawson even seems to agree that this has been the traditional view of economic method; he not only cites Mill as an advocate of this view, but also Neville Keynes (1989a: 63). He must also believe that tendency laws dominated the practice of economic science, at least until the middle of the twentieth century, since he cites Mill, Marx and Marshall (1989a: 62) all as practitioners of this view.

Hausman's position is particularly relevant to this discussion about the philosophical vision behind contemporary mainstream economics. Hausman presents what is basically the Millian view that economics is an 'inexact and separate science'. He argues (following Mill and Cartwright 1989) that causal laws 'are not mere correlations among features of human action', but that 'tendencies are the causal powers underlying the genuine regularities that inexact laws express' (1992: 127). He then discusses four separate notions of tendencies and the associated concept of an inexact law (1992: 127–131). None of these four notions exactly captures the way that Lawson uses the term tendency, but the point is that for Hausman, like Lawson, the 'laws' of economics are tendency laws. Hausman later goes on to argue that such laws can be rationalised in (weakly) empiricist terms, but he never abandons the basic Millian view. Much more important than his argument that such tendency laws can be reconciled with some version of empiricism is the fact that Hausman consistently argues that *the Millian view is the best characterisation of what economists actually do in their science* – not just Mill, Marx and Marshall, but contemporary mainstream (even general equilibrium theorists) as well.

> This [Mill's] vision of economics as a separate science, although not often expressed in this terminology, remains, . . . central to contemporary microeconomics. The whole project of microeconomics and general equilibrium theory presupposes that a single set of causal factors underlies economic phenomena and determines their broad feature. Other relevant causal factors are regarded as disturbing causes.
>
> (Hausman 1992: 225)

Thus not only do we see that contemporary mainstream economics is not driven by the philosophy of empirical realism, it is driven by roughly the same Millian tendency law vision that Lawson advocates. This is clear from our examination of the type of theoretical work that mainstream economists actually do, as well as what the best methodological commentators have said about that economic practice.

Conclusion

I believe that I have made a solid case against Lawson's interpretation of neoclassical economics and I will leave it to the reader to speculate about how this criticism impacts Lawson's more general philosophical programme. I have just two concluding points to make about this misreading.

First, Lawson tries to make a strong case that the transcendental realist framework will serve as solid ground for a *critique* of mainstream economics. In Lawson (1989a) the argument is packaged as a way of helping us understand (as legitimate) Kaldor's criticism of Walrasian general equilibrium theory; more specifically, Lawson argues that if we understand transcendental realism (Kaldor's philosophical position) we will understand why Kaldor thought that the abstractions of general equilibrium theory were of the 'wrong kind'. This argument does not work; transcendental realism does not help with such matters. The practical problem with transcendental realism – the practical problem with any such Aristotelian or essentialist philosophical framework – is that it provides almost no information about how one practically chooses between two theories. It is an ontology, a theory of the nature of being, and not a theory that provides any practical guide to determining what the nature of being actually is. Trying to use such an ontological framework to choose between two ostensibly scientific theories is a type of 'ontological fallacy' that is essentially the reciprocal of the 'epistemic fallacy' that Bhaskar discusses. The fact that Lawson can consider the economic theories of Mill, Marx, Marshall, Kaldor, and Hayek (1994b) to all be consistent with transcendental realism is testimony to the fact that such an ontological view does not help us choose between or among theories in any practical sense. When Lawson argues that 'Clearly assumptions such as universal perfect competition, linear expected profit functions, Cobb-Douglas production functions, etc. are not intended to capture the mode of operation of real economic mechanisms' (1989a: 74), he is simply asserting his own view of what is *real*. Many neoclassical economists *really do* view such conditions as the conditions that *really are the relevant underlying* conditions in a competitive market economy (at least as Hausman would say 'inexactly'). Lawson and the neoclassical economist can have exactly the same view of the type of things (real underlying causes) that one should be looking for in economic science, and yet disagree totally about what those real underlying causes are. Attempting to find some common ground for deciding which are, and which are not, the real underlying causes is of course how Western intellectual life came to be obsessed with epistemology; mere ontological frameworks don't help, and when one tries to use ontology for such purposes all one ends up doing is reproducing their own beliefs about what is and what is not legitimate knowledge (Kant is a case in point).

Second, there seems to be a certain irony involved in Lawson's view about mainstream economics. The irony is that Lawson appears to have applied the same epistemological realism at the meta-level that he criticised so harshly at the scientific level. To see this incongruity consider the question of how a good realist of the sort that Lawson endorses would go about explaining the theoretical behaviour of the economics profession. It seems that such a realist would look for the real underlying causes, the generative mechanisms, behind the (transitive) phenomena of day-to-day professional life in economics. The fact that most economics papers have econometrics and other 'empirical results', and the fact that the rhetoric of the discipline (what economists say they are doing) is all about testing, prediction, and operationally meaningful propositions, would not be the main interest of such a realist. All this is surface phenomena, event regularities on the surface of economic professional life; the transcendental realist would not stop here. What the realist would want to do is to *explain* this surface phenomena of empirical 'results' and econometric 'tests' – to find the underlying causal mechanism that generates this empirical phenomena of disciplinary empirical practice. There are of course many possible stories about what the relevant generative structures might be, but the point is that *Lawson never really asks the question*. He seems entirely content to stop at the empirical level and assume that economists really are doing exactly what their surface behaviour suggests they are doing – trying to describe accurately the event regularities in economic life. One would expect a transcendental realist to look deeper.

Notes

1 A partial list of this work would include Lawson (1989a, 1989b, 1994a, 1994b, 1994c, 1995 and 1997). Lawson also discussed realism and instrumentalism in his earlier work (1981 and 1983 for instance) but his argument was different than (and perhaps even the reverse of) the one presented in his later, Bhaskar-inspired, writing.

2 I will follow Lawson and others in using the term 'transcendental realism' for Bhaskar's general philosophical position, and 'critical realism' for the application of that programme to the social sciences by Lawson and others (see Collier 1994 and Jackson 1995).

3 Bhaskar (1978, 1987, and 1989); also see Collier (1994).

4 I will follow Lawson in freely substituting the term 'positivism' for the combination of Humean empiricist epistemology and empirical realist ontology. This does not imply that I endorse Lawson's use as the only, or even most appropriate, use of the term 'positivism'.

5 The one mainstream economist that is repeatedly cited as evidence for this positivist reading is Frank Hahn. It is not clear why Lawson considers Hahn to be an authority on the philosophical foundations of neoclassical economics. Hahn obviously made a number of very important contributions to 1960s' and 1970s' general equilibrium theory, but he has never demonstrated anything more than passing interest in the issues relevant to the epistemological appraisal of economic analysis. This, combined with the dismissive attitude exhibited in the few places where Hahn has mentioned methodological issues, makes him a particularly unreliable source for philosophical evaluation (and he is effectively Lawson's only source). One could learn from Debreu's comments about the Bourbakian programme (Weintraub and Mirowski 1994) or from Thomas Mayer's (1995) discussion of monetarist methodology, but not from Frank Hahn's miscellaneous methodological musings.

6 Lawson frequently mentions Walrasian general equilibrium theory as an example of mainstream or orthodox economics, but his primary discussion is contained in one of his earliest papers employing the Bhaskarian language (Lawson 1989a).

7 See for example Deaton and Muelbauer (1980), Gilbert (1991), or Keuzenkamp and Barten (1995).

8 Of course it may be that such (Humean) empiricism has never represented the guiding principle behind any type of scientific activity, natural or social, but that is a separate issue.

9 One could add that empiricists in other social sciences also have the same criticism of economic theory (Green and Shapiro 1994, for example).

10 In fact Hollis and Nell (1975) appears to be the *only* book on economic methodology in the last twenty years (and there have been many such books during this period) that *does* agree with Lawson about the type of empiricism that dominates economics – and even Hollis eventually seemed to change his mind on the matter. Hollis (1996) presents four basic approaches to social science (empiricist, post-empiricist, realist, and interpretative) and neoclassical economics is clearly listed as 'an example of explanatory realism' (p. 367), not as empiricism.

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