EXPLAINING THEORY CHOICE: AN ASSESSMENT OF THE CRITICAL REALIST CONTRIBUTION TO EXPLANATION

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Critical realism is now an important body of literature in the philosophy of the natural and social sciences (cf. Archer *et al.*,1998). It currently has a notable following in economics (see Fleetwood,1999), where its main and most able proponent is Tony Lawson. Lawson's much discussed and deservedly acclaimed *Economics and Reality* (1997) offers those who do not wish to follow the neoclassical mainstream in economics a comprehensive methodological manifesto for overcoming the discipline's ills. A key theoretical component in this manifesto is 'explanatory power'. In the first two sections of this paper, I examine the critical realist account of explanation in the context of Lawson's work. The problems which his work contains lead me to consider Bhaskar's writings on theory choice in the two sections thereafter. The theme of this paper is important for it is according to their relative 'explanatory powers' that theories should be accepted or rejected. The issue comes particularly to the fore in a discipline like economics, the ranks of which carry what many regard as an alarming degree of disagreement. As well as highlighting internal problems in critical realism's account of explanation, I consider also the relationship between philosophical treatments of explanation and their relation to social scientific practice. Can critical realism guide the latter? Only if it does so does it find its *raison d'être*.

Thanks to the frequency with which critical realism is discussed in this journal, I can outline the realist social ontology and the model of explanation which follows therefrom with due brevity. I do so in section I using Lawson's work as my source. I then subject his concept 'explanatory power' to critique. My objections are, first, that the concept is not clearly defined, leaving the reader wondering what explanatory power actually is. Secondly, Lawson conceives 'explanatory power' quantitatively – theories have *more* or *less* of it. This causes him to overlook the possibility that there are different *types* of explanation which do not admit of quantitative comparison, something which one discerns only by treating explanation less abstractly than Lawson. And it is exactly this abstractness which renders critical realism unhelpful to practising scientists in the business of choosing theories. I demonstrate this with an example at the end of section I. In section II, I discuss Lawson's views on 'economic theory', which stress the centrality of the deductivist method in economics. I argue that Lawson lays too much emphasis of this element which precludes him from seeing the importance of other components of economic theory which are not reducible to its deductivist methodology. In particular I note those elements in economic theory – neither *a priori* true nor *a posteriori* demonstrable – which are as important to the discipline as its deductivist method, but not reducible to it. Ignoring such elements and their epistemological status obscures the difficulties in convincing economists of the explanatory weakness of their theories. At the end of the section I address the question of the subject to whom Lawson's comments might be addressed. Here the normative nature of his account becomes conspicuous.

The unanswered questions in Lawson's work lead me to discuss Bhaskar's analysis of theory choice. Bhaskar's target is the 'superidealism' of Kuhn and Feyerabend. By refuting this position, Bhaskar seeks to establish the possibility of rational theory choice. However, he argues not just for the *possibility*, but also the *reality* of rational theory choice. This leads to problems in his descriptive hypothesis '*that* scientists choose theories rationally' for the reason that he is unable to justify this claim by criteria independent of the *de facto* choices of scientists. I then go on, in the final section, to consider the normative implications of his theory with the aid of an example, where it is shown that applying Bhaskar's criteria for theory choice lead to paradoxical conclusions. Such conclusions result from misunderstanding the status of particular beliefs and elements of explanatory schemas which, although neither empirically nor *a priori* justifiable, are inescapable for any *Weltanschauung*, scientific or otherwise.

I. Explanation and Explanatory Power

The critical realist position consists in a transcendental deduction of a social ontology according to which reality exists independently of theorists' conceptions about it. I must here introduce two 'technical terms' which form the poles of a distinction between the 'intransitive' objects of science ('reality' itself) and the 'transitive' dimension of knowledge about those objects. Planets, quarks, unemployment and electoral systems are all intransitive objects; they are distinct from the knowledge science produces about them. The transitive dimension, on the other hand, consists of 'facts, observations, theories, hypotheses, guesses, hunches, intuitions, speculations, anomalies, etc.'(Lawson, 25).[1] Furthermore, the intransitive dimension is said to be *stratified*. Stratification means that reality consists of distinct 'layers' which are neither synchronised with, nor reducible to, one another; how an event appears to observers (the 'level' of the empirical) will differ according to the perspective of the observer. This is the basis of the distinction between the 'empirical' and 'actual' (the event itself, not simply as viewed from a particular perspective). Of greater significance is the distinction between the foregoing strata and a third, namely the 'real' which posits a domain of structures and mechanisms which 'underlies' 'surface phenomena' (21-2). Lawson illustrates this as follows:

> the world is composed not only of such 'surface phenomena' as skin spots, puppies turning into dogs, and relatively slow productivity growth in the UK, but also of underlying and governing structures and mechanisms such as ... viruses, genetic codes and the British system of industrial relations (22).

Science, according to critical realism, identifies 'the structures and mechanisms ... that govern or facilitate the course of events'(23). How does it do this?

Social science finds its point of entry in the observation of partial regularities, e.g., ‘women look after children more often than men do’ or ‘average employment rates in western industrial countries are higher in the 1990s than the 1960s’ (206). These partial regularities are of particular significance when they are ‘contrastive’, i.e., when they occasion surprise (206,210-1). In such cases, it is ‘*prima facie* plausible ... that there are systematic and identifiable mechanisms in play which social science can uncover’(207). The mode of inference involved in uncovering these mechanisms is ‘retroduction’, the goal being

> to posit a mechanism ... which, if it existed and acted in the postulated manner, could account for the phenomenon singled out for explanation. Not much can be said about this process of retroduction independent of context other than that it is likely to operate under a logic of analogy or metaphor and to draw heavily on the investigator’s perspective, beliefs and experience (212, cf. 24).

Once a mechanism has been posited, the task remains to substantiate its existence empirically and eliminate alternative hypotheses about its nature (243). It is in these steps which explanation consists.[2] Explanation therefore moves from intransitive phenomena to pre-existing knowledge and concepts in the transitive realm through which the mechanisms which underlie and generate the phenomena are postulated and (hopefully) discovered (Bhaskar,1986:53-4). Discovery thus reveals deeper ‘layers’ of reality which explain the shallower and must, once discovered, themselves be explained by yet deeper ‘layers’. There is no *a priori* reason to believe that reality’s depths has limits and therefore that science will ever come to an end.

Having adverted to the existence of competing explanations, Lawson discusses how one might differentiate between them. Controlled experiment does not stand at the disposal of the social scientist. Furthermore, the openness of the social world entails that an underlying mechanism (at the level of the ‘real’) will not *alone* determine the course of events (the ‘actual’) which is observed by the social scientist (the ‘empirical’). The three ontological ‘levels’ are not synchronised with one another as they are in a controlled experiment in which a mechanism can be isolated from others or in which those others are held constant in their effect.

> Because actual events or states of affairs may be co-determined by numerous, often countervailing, mechanisms the action of any one mechanism ... may not be directly manifest or ‘actualised’(22).

Our inability to predict in exactly which circumstances a mechanism in the social world will operate renders ‘event-predictive accuracy’ useless in testing explanatory adequacy. The appropriate criterion according to which explanations can be compared is therefore ‘explanatory power’(213) (and *only* explanatory power (Bhaskar,1989:83)).

Lawson elucidates the concept ‘explanatory power’ in the following ways:

> Theories can be assessed according to their abilities to illuminate a *wide range* of empirical phenomena. And typically this will entail accommodating precisely such contrastive demi-regs [partial regularities] as are recorded or can be found (213).

> [W]e are ultimately interested in the *relative* performance of hypotheses whatever the relevant selection criteria. It is thus straightforwardly reasonable to search out that theory whose consequences appear mostly born out and which illuminates the widest range of empirical phenomena including any intersection upon which all competing theories have some possible bearing. Relative explanatory power is likely to be sufficient as well as appropriate here (213).

Elsewhere he repeats the connection between explanatory power and ‘illuminating’ features of experience (60) and writes in terms of theories being ‘outperformed in terms of explanatory power’ by other theories (244). Lawson is by no means sanguine about the problems which the assessment of explanations poses and repeatedly stresses the difficulty of clarifying ‘explanatory power’ outside the context of an example (213-4, 321,n.6). Here we confront a problem: Discussing explanation abstractly is, indeed, difficult; but critical realism is committed to such abstract discussions because it seeks to defend a *mode of explanation* based on insights into the general nature of the world. It does not judge the results obtained by scientists employing this mode of explanation:

> [Critical realism] is not, nor does it license, either a set of substantive analyses or a set of practical policies.... It is not a substitute for, but rather helps to guide, empirically controlled investigations into the structures generating social phenomena (Bhaskar,1989:3; cf. Lawson, 48).

Hence we find critical realists defending the work of theorists as diverse as Marx, Keynes, Hayek, Kaldor and Commons, whose substantive analyses could hardly differ more (cf. Fleetwood,1999). Nevertheless, Lawson believes he tells us something about explanatory power, albeit at a high level of abstraction. Let us enquire thereafter.

One is obliged to ask what is meant by ‘outperform’, ‘relative performance’, ‘illuminate’, to ’bear out’ or ‘accommodate’ empirical phenomena. We are told that one theory may be rejected in favour of another when it is ‘outperformed in terms of explanatory power’(244). In road tests, one talks of a car ‘outperforming’ another – it may have quicker acceleration due to greater horse power or it may be safer because it ‘holds the road’ better than a second due to its lower centre of gravity or the greater tread of its tyres. There are three elements in the elucidation of ‘outperforms’ here:

(1) An initial statement: ‘Car A outperforms car B’;
(2) A reference to the *respect in which* A outperforms B: safety/holding the road;
(3) The identification of a mechanism thanks to which A outperforms B: greater horsepower/ greater tyre tread.

In Lawson’s account of explanation, only the first two elements are present: Theory A may outperform theory B (1) with respect to their explanatory powers (2). But no mechanism is given thanks to which A has more explanatory power than B. This argument would be sufficient only if ‘explanatory power’ were a self-explaining category. But it isn’t. In effect, we are told little more than that theories should be chosen if they have more of something undefined than other theories. Lawson fails to heed the standards which Bhaskar(1989:19) imposes on scientific theory construction, standards which hold as much for a theory of explanation as they do for any other theory:

> the hypothetical entities and mechanisms [which in this case give theories their ‘explanatory power’] imagined for the purposes of theory-construction [a theory of how theories should be chosen] must initially derive at least part of their meaning from some other source (if they are to be capable of functioning as explanations at all).

But where is this meaning from another source; where, that is, is the analogue with ‘having quicker acceleration due to greater horse power’? Lawson cannot provide this meaning because his discussion is too abstract. Yet his adherence to critical realism hinders his descent from the ontological heavens to the worldly quagmire of scientific practice, where ‘explanatory power’ could be elucidated. This is not the only problem to which the abstractness of his discussion gives rise.

Consider, once again, the terms with which Lawson tries to elucidate ‘explanatory power’. He talks of:

- theories *illuminating* empirical phenomena,
- *accommodating* partial regularities,
- relative performance of hypotheses,
- theories’ consequences being *borne out*, and
- theories *outperforming* others.

Although the terms are vague, they imply that one can conceive of ‘more’ or ‘less’ explanatory power. A theory’s explanatory power thus appears to be something which one can compare *quantitatively* to that of another theory: theory A might ‘illuminate’ *more* empirical phenomena than theory B, it may ‘accommodate’ a *greater number* of partial regularities, or perhaps *fewer* of its consequences will be ‘borne out’. Only the notions ‘outperforming’ and ‘relative performance’ are ambiguous on this issue, but they are too nebulous to shed much light on explanatory power at all. What Lawson overlooks with the use of such terminology is not only that one theory may illuminate *more* phenomena than another, but also that it may well do so in a *completely different way* to that of its rival(s). He does, when discussing ‘relative performance’(213), mention ‘relevant selection criteria’, as if explanatory criteria may differ for different theories. However, this is not made clear. Furthermore, it is insufficient in acknowledging the existence of qualitatively different *types* of explanation because Lawson goes on to say that ‘relative explanatory power is likely to be sufficient as well as appropriate here’. All qualitative differences are effectively swallowed up by the quantitative bias in his work.

Take the recent work of macroeconomist Alan Blinder (1991; 1998), who proposes a new approach to investigating an old question: ‘Why are prices sticky?’ Dissatisfied with orthodox approaches which have thrown little light on the matter, Blinder questioned managers who take price decisions in large firms. In interviews, he formulated a number of economic theories on price-stickiness in ordinary language to ascertain if any of them 'matched' actual pricing policies. The methodological problems and results of Blinder's work are not the topic of this essay. I wish rather to consider comments on his work by Herschel Grossmann (1991). Blinder, so Grossmann holds, does not distinguish between 'the gathering of facts about behaviour and the *explaining* of facts about behaviour'(*ibid.*:99). He then goes on to state that 'explaining facts' entails

> a specification of preferences and constraints and the derivation of implications [which] involves the solution of a constrained-maximisation problem.... [A] central part of the research programme of neoclassical economics ... is an attempt to discover the particular constrained-maximisation problem that best fits the facts (*ibid.*).

The 'theories' which Blinder offers his managers are not of this form and therefore cannot, in Grossmann's opinion, be explanations.

Of what use might critical realist criteria be in judging whether Blinder's theory has more explanatory power than theories of the type suggested by Grossmann? Both theories can be made to fit Lawson's explanatory schema in terms of observing surface phenomena, positing possible underlying mechanisms, thus leading to attempts to identify the existence of such mechanisms. But this is too abstract to be of use ; if one looks back at the list of terms employed by Lawson to elucidate the concept 'explanatory power', it is not clear which of the approaches discussed above would 'outperform' the other or accommodate the greater number of partial regularities, etc. For Grossmann, one should point out, it is quite clear which theory is better, but only because he has a particular understanding of explanation which not everyone shares. So long as one is unwilling to intervene in such concrete matters, one will be unable to address these issues appropriately. What would Lawson say to this? To answer this question, it is necessary to take a look at his account of 'economic theory'.

II. Deductivism and the Nature of Economic Theory

Far from being indifferent to methodology, as some of its protagonists recommend, economic theory is, according to Lawson, driven by a particular methodological stance, *deductivism*, which conceives theorising in terms of deducing law-like regularities from initial conditions (16-7); deductivism is said to be 'constitutive' of economic theory (103) and 'is a feature ... that does not turn on the choice of substantive premises'(91). The meaning of 'constitutive' is not entirely clear, but it is obvious that Lawson thinks economic theory would be of a different nature were it to relinquish its deductivist methods. Lawson asks: how would the world have to be for deductivism to be applicable? Two conditions would have to obtain:

i) individuals would have to have an unchanging internal structure and be so constituted that, in specific conditions, only one course of action is possible.
ii) those factors not specified in the initial conditions would have to have an unchanging influence on the outcome.

These two ‘conditions for closure’ (respectively the ‘intrinsic’ and ‘extrinsic’) must hold if one is to predict the event regularities so central to economic theory (77-9, 98-100). Lawson is at his boldest in arguing that the substantive assumptions and axioms of economic theory derive from attempts to conceive the world as if these two conditions were valid. Hence, the individual is the unit of analysis in economics and actions are explained by agents maximising a function under constraints which provide a unique outcome (100).[3]

Lawson is right about the tenacity of deductivism. He is also right that the tools of economic theory effectively ‘close’ the world, thus allowing the theory to assume its deductive form. However, he goes too far in holding that the assumptions and axioms of economic theory are (mere) derivatives of economists’ adherence to deductivism. Consider the maximisation concept, about which Lawson holds two theses:

1) the use of maximisation as a tool of analysis is a consequence of economists’ adherence to deductivism (100);
2) deductivism is independent of substantive premises (91).

It is clear that Lawson is discussing maximisation *qua* formal, analytical technique, that is, devoid of content and compatible with an unlimited number of substantive premises. In economics, this analytical technique acquires its substantive flesh in the form of *utility maximisation*, and the postulate thereby becomes a *motivational* one. *Qua* theory of motivation, the utility concept has a long history stretching back before the dominance of deductivism in economic theory.[4] If Lawson is right, then it is not specifically *utility* maximisation which is integral to economics (because deductivism is independent of substantive premises), but maximisation as such (being derived from deductivism). Is this a plausible portrayal of economic theory?

To answer this question, I consider (and contend) Kenneth Arrow’s(1986:70) claim that utility maximisation is ‘not in principle essential’ to economic theory, a claim which Lawson obviously shares. Arrow insists that ‘some conditions ... be laid down for an acceptable theoretical analysis of the economy’ but these need not involve utility. He gives a rather casual example of habit-dependent consumption to prove his case; the example he chooses is, as he says, ‘different from utility maximisation’, but it is nevertheless a maximisation problem (*ibid.*:69); whereupon one must ask: What is the difference between ‘maximisation’ and ‘utility maximisation’? Or: What is ‘utility’? This is a question which Arrow, like the majority of his colleagues, does not pursue. Broadly speaking, there are two options:

a) utility is the highest, ‘most complete’ end of action.[5] This in no way rules out the pursuit of other ends, e.g., profit, votes, moral values, but these are subordinate to the end of utility;
b) utility is a narrower notion referring more closely to *self-interest*.

With a), the difference between maximisation and utility maximisation is unclear: if *all* ends were subordinate to the ultimate end of utility, then everything is utility maximisation; the concept becomes so all-encompassing that it is effectively empty. This is avoided by b) at the cost of an implausible restriction on human motivation, which has long been criticised (see Sen,1977; Hirschman,1982). Economists have responded to such critique by trying to incorporate other types of motivation under the banner 'utility maximisation' (see, e.g., Becker(1976); Hirschleifer(1977)). Such moves have widened the scope of the postulate which thereafter floats somewhere between a) and b).

To support his case that utility maximisation is not essential to economics, Arrow (1986:70) cites examples of theories which are 'hard to fit into a rational framework [of maximisation]'; nevertheless, one cannot miss the fact that economists try to do precisely that, as is evinced in attempts to provide micro-foundations for Keynesian economics, or in interpretations of rule-following, habits and bounded rationality in terms of maximisation. This shows just how deeply entrenched *utility* maximisation is: far from being 'inessential', it is near inescapable.[6] How does this bode for Lawson's thesis?

If, as Lawson holds, it is deductivism which is 'constitutive' of ('essential to'?) economic theory, then one must ask why it is *utility* maximisation which has held economists in thrall and not the maximisation of some other variable. Utility maximisation certainly makes economic theory highly 'tractable'(Sen,1987:69), but so would the maximisation of a string of other values to which the deductive mode of inference is indifferent. So, why utility? The term certainly has a readily graspable meaning;[7] it also has a certain *prima facie* plausibility (compare: agents maximise altruism, sentiments of sympathy, pints of beer, disutility). I suggest, however, that beyond this, the term is part of a *Weltanschauung* which had its origin in the seventeenth- and eighteenth-centuries and grew up alongside the institutions of a capitalist economy (see Hirschman,1997). It has since become a part of the metaphysics of economics, a taken-for-granted assumption which most often goes unchallenged. Likewise with the individual being the unit of analysis, another facet of economic theory which Lawson lays at the door of deductivism (100).

These substantive, but non-demonstrable elements of a science are not peculiar to economics. They are what Kuhn calls the 'metaphysical parts of paradigms', one of his examples being: 'Molecules of a gas behave like tiny elastic billiard balls in random motion'. Kuhn points out that such theoretical elements supply scientists with 'preferred or permissible analogies and metaphors. By doing so they help to determine what will be accepted as an explanation'(Kuhn,1996:184). Holton's(1973; 1993) analyses such elements under the name 'themata', i.e., preconceptions which are neither analytical nor empirically derived. Examples are the belief that the universe is mechanistic, that nature is teleological, that matter is constituted of elementary particles (Holton,1973:24-5). Themata are rarely explicitly thematised, but are a necessary condition for deriving empirical hypotheses. Deductivism in economics is a 'methodological thema', likewise maximisation; utility, however, 'a thematic concept'(Holton,1993:9). Lawson is well aware of the thematic status of deductivism in economic theory;[8] but he fails to draw a distinction between such methodological themata and thematic concepts which cannot be accounted for methodologically. It is this which led me to say that he 'goes too far'(p.7 above) in attributing to deductivism more than that for which it is to blame. Lawson is blind to the thematic tier of substantive hypotheses which stand 'between' the pure logic and the theoretical results of science.

Let us return to the dispute between Blinder and Grossmann: why are such disagreements unlikely to be cleared up by Lawson's methodological remarks? Lawson's suggestion that maximisation is a human 'potential' which may be exercised and possibly actualised (see footnote 3 above) would involve demonstrating the existence of maximisation empirically rather than enshrining it as an axiom or an assumption. This would have very radical implications for economics not least because there would, no doubt, be many occasions on which agents either did not maximise or tried but failed. But this is already a completely different ('de-thematised') understanding of maximisation to that found in economic theory. Boland (1981:1036-6) captures the latter well when he writes that each investigation poses the challenge

> whether it is possible to show that the phenomenon [under investigation] can be seen as a logical consequence of maximising behaviour – thus maximisation is beyond question for the purpose of accepting the challenge.[9]

That Lawson is prepared to permit the maximisation concept only if there are good empirical grounds for doing so shows just how far he stands from economic orthodoxy. But Lawson seeks to subject economic theory to his critical realist themata, which are not independent of his explanatory schema. Consider the following critical realist themata:

- scientific laws refer to the structures of things and 'their characteristic modes of activity' (24);
- people and things possess 'powers' and 'capacities' which may be 'emergent' (21,106, 176);

The difficulty in negotiating 'thematic opposites'(Holton,1993:18-9) is well-known and is something to which I return in the final section;[10] just as the search for 'capacities', 'tendencies', etc. is integral to everything which Lawson is prepared to consider scientific, so is maximising utility integral to all that economists are prepared to consider economic theory. One can see that Lawson cannot understand utility maximisation in the same way as an economist does; there are deep thematic differences between the two. And Lawson's themata are components of Lawson's criteria for theory choice. It is thus obvious that economists will feel that they cannot receive a fair hearing if economic theory is judged by critical realist criteria. It would therefore be unsurprising were Lawson's alternative approach to be met by the same reaction as Blinder met from Grossmann.

I wish now to turn to a final question which lurks in Lawson's discussion of explanation without being addressed explicitly by him: *Who* is to judge which theory has greater explanatory power? Consider the two quotations on theory choice from page 213 of his book which I cited above (p.3): the first is written in the passive voice; the first sentence of the second is written from the perspective of an undefined 'we'; whilst no indication about the subject of the two sentences which follow is given at all, appeal being made nevertheless to 'straightforward reasonableness' and the sufficiency of explanatory power as a criterion in settling disputes between competing theories. Whom is Lawson addressing here, who is to make these choices? Recall that critical realists renounce the claim to endorse particular substantive analyses (see above p.4). Being a philosophical ontology, critical realism reserves itself the right to criticise science when the latter's ontological presuppositions conflict with the ontology of critical realism; but this is *philosophical*, not substantive, criticism (Bhaskar,1989:24). Questions of theory choice, which are necessarily concrete, are left untouched by philosophical critique. Hence it cannot be philosophers whom Lawson is addressing; their role is to offer ontological guidance and not to make theoretical choices. This leaves only one alternative: that practitioners in the field are to decide. Yet Lawson cannot appeal to economists because he finds their standards of theory choice wanting. It is quite clear from his heavy use of modals in the passages which I have cited, that he is trying to *convince* us that his methods are *better* than those currently employed by the bulk of economists. His account is therefore normative, not descriptive. But if the choices which economists themselves make are the wrong ones, somebody must tell them which are the right ones and why, a task which Lawson must abjure because of the high level of abstraction at which he operates. Lawson's general explanatory schema is too abstract to guide economists in choosing. Such questions, Lawson candidly states, necessarily embroil us in 'context': 'There can be no context-independent account of what is meant by [empirical] adequacy' (321,n.6). But if he is questioning the authority of most contexts in economics, where are his criteria to come from? These issues will arise more clearly in Bhaskar's writing on theory choice to which I now turn in order to gain a better hold on the critical realist approach to explanation and its problems.

III. The Search for Rationality in Science

For Bhaskar, the distinction between intransitive objects and transitive knowledge, and the stratification of reality, are of prime import. They are essential to his thoughts on theory choice and incommensurability which aim, amongst other things, to refute 'superidealism', a thesis ascribed to Kuhn and Feyerabend. Superidealists deny the distinction between the transitive and intransitive dimensions; this denial implies that a change of paradigm brings about a change of world. Bhaskar holds this position to be indefensible: if theories are incommensurable, as Kuhn and Feyerabend hold, there must be something *about which* they are so, namely an intransitive object of investigation. For to be dubbed ‘incommensurable’, theories must overlap referentially (intransitive dimension) even if the sense of their respective terms (transitive dimension) differs. In short, incommensurable theories *clash*, they do not merely *differ* (Bhaskar,1986:74-9). If this were not the case, we would have to concede that, say, monetarism were incommensurable with high-energy physics, which is patently absurd. Thus, the very possibility of incommensurability ‘depends upon the explicit recognition of a philosophical ontology or intransitive dimension’(Bhaskar,1989:19) which is distinct from the transitive dimension of knowledge.

Having accepted the possibility that theories be incommensurable, Bhaskar(*ibid*.:73) asks whether we can still retain the possibility of rational choice between two theories (even with no mutually shared meanings). His answer is ‘yes’ and he presents us with a criterion for doing so which he calls ‘[L]’ (by virtue of its Lakatosian flavour):

> [L]: A theory T_c is preferable to a theory T_d, even if they are incommensurable, provided that T_c can explain *under its descriptions*, almost all the phenomena that T_d can explain under its descriptions, plus some significant phenomena that T_d cannot explain (*ibid*.:73; see also 1989: 19,32).

It is important to be clear on that for which Bhaskar is arguing here: the transitive-intransitive distinction allows for the *possibility* of [L]; it in no way necessitates that one choose theories according to [L]; indeed, a large array of modes for theory choice is compatible with the transitive-intransitive distinction, including the supposedly ‘irrational’ modes about which Kuhn and Feyerabend supposedly write. Bhaskar has thus established the *a priori possibility* that theories can be chosen rationally. And that is all he requires to refute superidealism; whether theories are *actually* chosen rationally is not something which philosophy – defined, and differentiated from science, by its transcendental methods (Bhaskar,1989:14) – can ascertain.

Before looking at the way in which Bhaskar develops his approach to theory choice, let us consider [L]. Two similarities with Lawson’s work are apparent: its abstractness and its quantitative bias. Regarding the latter, terms like ‘almost all’ (remarkable for its vagueness) and ‘plus some ...’ indicate that we are in the realm of ‘more’ and ‘less’. Although [L] is explicit about the existence of different types of explanation (in that T_c explains phenomena under *its* descriptions and T_d under *its*), these differences are nevertheless disregarded in choosing between them, where, to repeat, it is a question of explaining *more* or *fewer* phenomena. Furthermore, when considering [L], one must distinguish between:

i) The *de facto* closure of scientific debates in which one of a number of rival theories is chosen.
ii) The possibility that the theory in question is chosen rationally (according to [L]).

iii) The possibility that [L] is consensually applied by all parties (Bhaskar,1986:75).

Neither ii) nor iii) can be inferred from i).

Bhaskar(1986:81) admits that his views represent a 'major over-simplification'; in order to remedy this, he introduces a new variable: the 'significance' of explanations (*ibid.*). This leads him to specify a 'qualitative clause', [Q], to supplement [L]. According to [Q], a theory is to be chosen in favour of another

> if it can either (a) identify and/or describe and/or explain a deeper level of reality; and/or (b) achieve a new order of epistemic (explanatory and/or taxonomic) integration, or at least show grounded promise of being able to do so (*ibid.*:82).

It is unclear exactly what [Q](a) adds to [L]. Bhaskar obviously thinks that explaining phenomena at deeper 'levels' of reality is different to explaining at the same 'level'. However, nothing in [L] excludes reference to different 'depths' of explanation. [Q](b) does add something not present in [L], something which is often mentioned in discussions of theory choice (cf. Kuhn,1977:321-2).

I noted above that Lawson's discussion of explanatory power is part of a critique of orthodox economics and contains a strong normative element. Is the same true of Bhaskar's views on theory choice which are orientated far more to the natural sciences? Bhaskar holds that his ontology is 'sufficient for the putative applicability of L'(1986:75) and that it 'sustains' rational theory choice (1989:20). These formulations are noteworthy not only for their imprecision;[11] they also merely repeat that it is *possible* that theories be chosen according to [L], something he has already established. If [L] is to have any force outside the realm of mere possibility, he must establish either its normative force or its descriptive accuracy, better still, both. Taken normatively, [L] would state that scientists *ought* to choose between theories according to [L] ('ought' implying 'can'). It is clear that Bhaskar has this sense in mind when positing [L]: it is not for nothing that he describes [L] as a criterion for *rational* theory choice (1986:73). But there also lurks an assumption that scientists *actually do* choose theories rationally; and since [L] is Bhaskar's only criterion for rational theory choice, one must assume that [L] is to be understood descriptively, too. He writes:

> The logic of scientific explanation ... gives, in as much as it is substantiated by detailed historiographical work, grounds for supposing some degree of the *actualisation*, i.e., the practical effectivity, of such criteria [[L] + [Q]] in the history of science (Bhaskar,1986:86).

A difference emerges here between Lawson and Bhaskar: the former, as I have already spelled out, does not endorse the explanations and theory choices made in economics. His account of explanation is therefore normative. Bhaskar, on the other hand, holds that critical realist criteria for theory choice are actually realised in the natural sciences. However, his tone is conspicuously tentative; he fails to tell us both *what* these 'grounds for supposing some degree of the actualisation' of his criteria are, and also to *which* 'degree' these criteria are 'actualised'. It is certainly something which cannot be philosophically demonstrated; hence the allusion to 'historiographical work'.

It is important to be clear why Bhaskar wishes to enlist the support of historians of science: he requires – *over and above his philosophical ontology* – evidence or justification for his belief that scientists do *in fact* choose theories according to [L] and [Q]. Only thus can he bridge the gap between possibility and 'practical effectivity'. He dare not trust the *de facto* choices of scientists without some independent empirical confirmation – scientists are, after all, fallible. Were he to rely on the *de facto* choices of scientists, the conclusion *that* scientists choose theories rationally would be a truism of the form:

- theory T_c was chosen in favour of theory T_d
- scientists' choices are generally rational (in the sense of [L])
- since T_c was chosen by scientists it must be have been rationally chosen.

If this argument were accepted all historiographic analysis would be superfluous, a position which Bhaskar wishes to avoid. Some of Bhaskar's followers are less cautious than he, leading to the following kind of historical anecdote:

> [W]hat Priestley called 'de-phlogisticated air', Lavoisier called 'oxygen'; Lavoisier could explain more by his theory about oxygen than Priestley could by his theory about de-phlogisticated air. Hence it was rational to choose Lavoisier's theory (Collier,1994:91).

With such examples, the history of science is a reconstructive, armchair pursuit in the service of rubber-stamping actual scientific choices.[12] But Bhaskar requires more. What can historians of science offer him?

This question returns us to one asked earlier: *who* is to apply [L] and [Q]? Unlike Lawson, Bhaskar(1986:74-5) not only stresses the importance of the existence of a subject who can apply [L]; he also addresses his theses to the adherents of rival theories, not just to third parties who stand 'outside' the debate. He asks whether the protagonists themselves are 'capable of consensually applying [L]', but leaves the question hanging (1986:86), perhaps wisely given the myriad examples of scientific dissensus, even in cases in which the scientists themselves have sought consensus (see Mulkay,1985, pt.I; Gilbert and Mulkay, 1982). And this does not damage Bhaskar's argument because the ability of scientists to apply [L] consensually is independent of the issue *whether* theories chosen are so according to [L]. What, then, of the appeal to historians who, according to Bhaskar, can help establish whether (or, rather, *that*) [L] is 'practically effective' in science? There are three problems with this idea:

1) It represents quite a responsibility for the historian, who is asked to adjudicate on disagreements between scientists who are infinitely more qualified than the historian in the relevant field. It is a task which many historians abjure due to their lack of competence in the field.
2) Disagreements between historians about whether a theory was rationally chosen are common. Perhaps historians themselves must apply [L] to their own disputes. But then

we will need yet more 'detailed historiographical work' in order to establish whether the historians have correctly applied [L]....

3) Historians of science often find that science is carried out in ways which scarcely resemble philosophers' preconceptions about science. If they rely too much on such work, philosophers would have to admit that some of our most cherished examples of science, the rationality of which few would like to question, do not in fact concur with the standards of rationality set by philosophers.

So it seems that historical studies will create just as many problems as they are supposed to solve. They will be highly inconclusive in determining whether theories are actually chosen according to [L]. Thus, the judgements of scientists, philosophers and historians are inadequate to the task of showing *that* theories are chosen rationally.

A last option is to by-pass these three groups of scholars and look at the success of scientific theories themselves. *Some* scientific theories, it could be said, are applied outside the laboratory, indicating that they must in some sense be correct and hence rationally chosen (Bhaskar,1989:16). And indeed, it would be absurd to behold the application of science whilst holding that scientific theories are chosen arbitrarily. But this ground is too shaky to provide a firm basis for Bhaskar's faith in science. There is no necessary connection between choosing theories according to [L] and [Q] and the fact that many scientific theories are sometimes successfully applied. Even on critical realist assumptions, the 'successful application argument' fails, as I now show.

When outlining the critical realist account of explanation above (pp.2-3), I noted that the process of discovery reveals ever deeper 'layers' of reality which, once discovered, become subject to explanation thus yielding phenomena at yet deeper levels. Knowledge of these deeper layers not only explains phenomena at a more superficial level, but may also correct knowledge about those more superficial layers (Bhaskar,1989:20). Scientific knowledge, according to critical realism, is constantly subject to revision; it does not always progress continuously but is subject to revision and rupture. This does not mean that knowledge of more superficial levels, which is corrected after we acquire knowledge of deeper levels, is simply or necessarily false. Rather, we can conceive situations in which theory A explains phenomena in a way which becomes superseded by a more adequate, 'truer', expression of the object, given by theory A^* (Lawson,244-5). Naturally, theory A can be applied outside the laboratory independently of the existence of A^*. This severs any necessary connection between successfully applying theories and choosing the 'best' one (according to [L]): theory A can still be applied after the discovery of A^*, which fact alone tells us nothing about its relative explanatory power.[13]

I conclude, therefore, that Bhaskar fails to defend his opinion that [L] provides a description of actual theory choice in science. What about the normative use of [L], can it possibly help scientists in making rational theory choices? I consider this question in the following section with the aid of an example.

IV. Explanatory *Weltanschauungen*

The example is familiar – taken from Evans-Pritchard's *Witchcraft, Oracle and Magic among the Azande* (1937). To begin with, I restrict my attention to Zande attempts to explain common *malheurs* which befall members of the group. In doing so, I am dealing with a practice – explanation – which is common to both scientific and Zande culture, although carried out very differently in each case.[14] I compare Zande and western scientific explanations according to Bhaskar's [L] and [Q] to see if these can help to determine which has the greater explanatory power.

When a Zande says 'witchcraft has made so-and-so ill [this] is equivalent to saying in terms of our own culture ... that so-and-so has caught influenza'(Evans-Pritchard,1937:64). Indeed, all events which are in any way irregular, and especially those involving misfortune, are explained by the Zande by referring to witchcraft. But:

> It is [only] the particular and variable conditions of an event and not the general and universal conditions that witchcraft explains. Fire is hot, but it is not hot owing to witchcraft, for that is its nature. It is a universal quality of fire to burn, but it is not a universal quality of fire to burn *you*. This may never happen; or once in a lifetime, and then only if you have been bewitched (*ibid*.: 69).

Zande explanation thus contains factors usually absent from scientific explanation. This is particularly evident in the example of an old granary which collapsed as a result of termites eating through the supports (Evans-Pritchard,*ibid*.:69-70). On the causal relation between the termites and the collapse, Zande and scientific explanation are in agreement. At the time of the collapse, people were sitting under it sheltering from the sun. Why did *this* granary collapse precisely at *that* time on *these* people? A scientific explanation would not attempt to explain why it was these people and not others who were unhappily positioned at the time of the collapse. Knowing that people often took shelter from the sun under such granaries might lead one to speculate that it was likely for people to be under this one at the time of collapse. But why precisely these events coincided in time and space is not something which is open to explanation. We would merely say that it was 'bad luck' or 'coincidence', which is to say that these matters lie outside our explanatory purview. Not so with the Zande:

> Zande philosophy can supply the missing link. The Zande knows that the supports were undermined by termites and that people were sitting beneath the granary in order to escape the heat and flare of the sun. But he knows why these two events occurred at precisely the same moment in time and space. It was due to the action of witchcraft ... Witchcraft explains the coincidence of these two happenings (*ibid*.:70).

As Evans-Pritchard points out, 'natural' and 'mystical' accounts of causation are not mutually exclusive. Yet the Scientific Revolution expunged the latter in the name of superior explanation.

How would these competing explanations fair in Bhaskar's schema? Under [L], the Zande explanation is plainly superior for it clearly accounts for *more* phenomena under its descriptions than its rival. Attributing the spatio-temporal coincidence of events to 'chance' or 'luck' does not constitute an explanation under science's own understanding of explanation. Under [Q], too, the Zande explanation must be deemed to have the advantage for it not only identifies a level of reality to which its scientific counterpart makes no reference; it also unifies in one explanatory scheme both causal and 'mystical' elements. The absurdity of the example is plain; nobody reading this will now be convinced that the Zande explanation of the granary incident is the superior of the two (presuming that the reader was not already of this opinion). And the obvious retort is that scientific explanations are *superior*. However, it is precisely the superiority of some types of explanation which is not addressed by Lawson and Bhaskar, who, it will be recalled, conceive differences in terms of more and less. What if I now lift the restriction I imposed on myself at the beginning of this section and permit my realist interlocutor the following objection: 'Yes, but this witchcraft business is absolute bunkum; you're not seriously entertaining the possibility that it might be true?' To which I would reply: 'Of course not'. But, following Wittgenstein(1989), one cannot simply say that the Zande *Weltanschauung* is a mistake; it is, far more, a fact of life for the Zande through which sense and meaning is given to their lives. And here we return to themata, which I discussed in section II.

That witchcraft is an explanatory factor for the Azande can be neither *a priori*, nor empirically proven. But utilising such an 'assumption' is not specific to the Azande: economists' use of utility maximisation is similar, that is, an intrinsic part of a *Weltanschauung* on which a mode of explanation is founded. I also listed some of the themata in Lawson's work which are opposed to those of economic theory. Themata are likewise apparent in natural science, e.g., a reliance on material causation and an eschewal of mystical and paranormal causes as explanatory factors.[15] Natural scientists are just as uncritical of their 'themata' as critical realists, economists and Zande here; they take material causation to be the only explanatorily legitimate form of causation, dismissing other types without ever examining them empirically (see, Hyman,1988; Kurtz,1988). This is not to say that thematic changes are impossible; indeed, the history of science is replete with them, e.g., the medical acceptance of the fact that psychological factors can cause physical symptoms (Freud,1905:15). But such changes do not simply arise when scientists ascertain that one theory has more explanatory power than another. The whole concept of explanation may change with such episodes. Themata play an important role in determining which explanations are recognised as valid by practitioners in the field. Hence, one cannot simply compare theories and their explanatory power according to a single set of criteria based on comparisons of more and less, as critical realists ultimately try to do. Unless far more attention is paid to different *types* of explanation and their related themata and *Weltanschauungen*, one will be unable to gain insights into explanation and theory choice, as the case of critical realism shows. Furthermore, one will be unable to produce criteria which might aid practitioners to scientific (or other) disputes in choosing the best theory.

NOTES

[1] Page references unaccompanied by author's name and publication year refer throughout to Lawson(1997).

[2] This is actually one type of explanation – the 'pure' or 'theoretical' – which is distinct from an 'applied' type (220-1,243-4). These types correspond respectively to Bhaskar's(1986:68) DREI and RRRE models of explanation.

[3] It is important to note that Lawson is not averse to the maximisation concept *per se*; but he insists that it be conceived as a 'potential' of human beings. Potentials *may* be exercised as tendencies in some situations, and *may* also be actualised in cases where countervailing tendencies allow. Economists make the mistake of conflating the 'real' with the 'actual', identifying a potential with its exercise and its exercise with its actualisation. Lawson imposes a constraint on theorising, namely that such potentials must at least be 'real possibilities' as opposed to pure 'fictions' which are not unknown to economics (106).

[4] 'Utility' is not necessarily to be understood in the narrow sense of selfishness; see the many attempts to incorporate altruism into the utility function, e.g., Becker(1976) and Hirschleifer(1977). The 'history' of the concept in economics is based in large part on a one-sided reading of Adam Smith (Winch,1978).

[5] 'Most complete' is used in its Aristotelian sense here, the most complete end being that which is pursued only for its own sake, not for the sake of a higher end (see Aristotle,1984:1097a31-5).

[6] '[I]t is certainly true that the assumption of 'economic man' relentlessly pursuing self-interest in a fairly narrowly defined form has played a major part in the characterisation of individual behaviour in economics for a very long time'(Sen,1987:69).

[7] Caldwell(1983:826) rightly criticises economists for not specifying what utility actually is. But nevertheless, the term picks out more than merely an undefined 'something'.

[8] Criticism of deductivism '*per se* is something that cannot, apparently, be comprehended' by economists. 'Nor is the supposed universality of deductivism ever really questioned'(Lawson,1994:260). Similar things could be said of utility maximisation.

[9] Boland(1981:1034) describes maximisation as part of the metaphysics of economists.

[10] See Holton's(1993:19) example of the thematic dispute between Einstein and Heisenberg.

[11] 'Sustains *the possibility* of rational theory choice' would be an accurate formulation of Bhaskar's meaning.

[12] Had Priestley's theory been chosen instead of Lavoisier's, and the stream of science which would have flowed therefrom been realised, the history of science would have looked quite different to how it actually does. But that is unlikely to have prevented Collier from informing us proudly that Priestley's theory was rationally chosen because it had more explanatory power than Lavoisier's.

[13] Unfortunately, critical realists pay scant attention to the application of scientific theories, be it successful or not. This is a regrettable gap in their theoretical interests. Good analyses of application are to be found in Hacking(1992), Latour(1983) and Rouse(1987), who examine the way in which the social and material relations 'inside' the laboratory are extended into situations 'outside'.

[14] Note that I am not suggesting that the practice of witchcraft *per* se is an explanatory endeavour; rather that it can be and is appealed to in Zande explanations of everyday affairs. I avoid judgements about the Zande belief system as a whole because of the problems in claiming that it is 'true' or 'false' as such (cf. Wittgenstein,1989).

[15] Hacking's(1992:50) examples include Kelvin's dictum regarding measurement and Galileo's adage that 'the author of the book of nature wrote the book of the universe in the language of mathematics'. See Holton (1973:24-5) for further examples.

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