

Review Articles

MORE CLOTHES FROM THE EMPEROR'S BARGAIN BASEMENT*

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I LAUDAN'S MODEL

In his recently published book *Progress and its Problems*¹ Laudan presents a model of rationality that is wide enough to cover 'all intellectual disciplines' (13)—but he explains it *via* a discussion of its 'most successful instance', science (13). The model is simple and apparently quite powerful. It is one of those ideas which at first sight seem hardly worth a glance (11) but which reveal their fertility when developed in detail.

According to Laudan (scientific) knowledge arises from the attempt to *solve problems* (13): 'science is essentially a problem solving activity' (11; *cf.* 66). This is the basic postulate. The postulate is explained by showing what entities are involved in problem solving and how the solutions are evaluated.

Problems are solved with the help of theories and *research traditions* which are sets of 'general assumptions about the entities and processes in a domain of study and about the appropriate methods to be used for investigating the problems and constructing the theories in that domain' (81—italics in the original).

Theories and research traditions are *evaluated* by their problem solving propensities (14). The evaluation is *comparative* (71)—'what matters is not, in some absolute sense, how effective . . . a tradition or theory is but, rather, how its effectiveness . . . compares with its competitors' (120): one chooses 'the theory (or research tradition) with the highest problem solving adequacy' (109).

Solving problems scientists 'need not and generally do not consider matters of *truth and falsity*' (24—my italics) and wisely so for the problem solving model works while truth models, partial truth models and probability models don't (127f).² Combining theory choice with problem solving adequacy entails that 'rationality is parasitic upon progressiveness' (125); there are not two ideas, reason and progress and the need to show how they are connected; there is just one idea of rationality where being rational already means making choices that are progressive (125).

* Review of L. LAUDAN [1977]: *Progress and its Problems—Towards a Theory of Scientific Growth*. Henley: Routledge and Kegan Paul. £5.95. Pp. 257.

¹ Numbers in brackets refer to pages in Laudan's book. [Publication of Professor Feyerabend's review has unfortunately been delayed by various postal and administrative hitches.—*Ed.*]

² The fact that truth is absent from the *general* standards of rationality does not preclude its appearance among the *specific* parameters of particular research traditions, such as that of Kepler. (*Cf.* 126).

The model splits rationality in two parts, a general framework that is said to be present in all cases of rational inquiry and 'specific parameters' which are time- and culture-dependent (130). 'The model argues that there are certain very general characteristics of a theory of rationality which are *trans temporal* and *trans cultural*, which are as applicable to Presocratic thought or the development of ideas in the Middle Ages as they are to the more recent history of science. On the other hand the model also insists that what is specifically rational in the past is partly a function of time and place and context' (130f). This dual aspect of rationality combines the aspirations of the philosopher who defends eternal rules of reason and the relativism of the historian who asserts that reason depends on time and context. So far a *general outline* of Laudan's model or rationality theory.

The presentation of the general outline is combined with a discussion of *specific features*. Laudan distinguishes between empirical problems and conceptual problems and emphasises the importance of the latter. *Empirical problems* become a challenge to a theory or a research tradition (they become 'anomalies') only when they have already been solved by some theory or research tradition (18; 29; cf. 21f: 'in appraising the relative merit of theories the class of unsolved problems is altogether irrelevant. What matters for the purpose of theory evaluation are only those problems which have been solved . . . by *some* known theory'). Being '*specific parameters*' (120) of rationality (see the end of the last paragraph) the conditions for the solution of empirical problems 'have evolved' (25) and they are occasionally quite loose when viewed from the standpoint of a logic freak (24). 'Assessing the importance of . . . anomalous problems for a theory has (therefore) to be done within the context of other competing theories in the domain' (38) and 'the importance of solving all empirical problems is not the same, some being of much greater weight than others' (40). *Conceptual problems* may be internal (consistency, ambiguity, circularity—49) or external (boundary conditions such as the condition of circularity in ancient astronomy up to and including Copernicus, Einstein's 'reality condition', more general ideas such as the idea of causality and so on) and they involve theories, research traditions, entire world views (61) as well as norms: 'every historical epoch exhibits one or more dominant normative images of science. It would be a serious mistake to imagine, as many historians do, that these norms are just the concern of the professional philosopher or logician' (58; cf. 164ff for details). Laudan stresses that tensions between *world views*, theories, norms which are either overlooked or pushed aside as irrelevant by positivists (a) *have* influenced science and (b) *have* influenced it in a *rational manner*, i.e. in accordance with the problem solving model: 'the overall problem solving effectiveness of a *theory* is determined by assessing the number and importance of the empirical problems which the theory solves and deducing therefrom the number and importance of the conceptual problems which the theory generates' (68—italics in the original). *Successful research traditions* lead, '*via* (their) component theories to the adequate solution of an increasing range of empirical and conceptual problems' (82; cf. 108f for details and 119f for a summary).

The model is not restricted to science. Metaphysics, theology, even the 'formal' sciences contain empirical problems (189ff, esp. 191 top): 'What has stood in the way of a recognition of the cognitive parity of the sciences and the non

sciences has been a simplistic identification of (scientific) rationality with experimental control and quantitative precision' (191); 'if there is any truth at all in the (positivistic) claim about the difference between the sciences and the non sciences . . . it will be found, not in the exclusive exhibition of progress by the sciences, but rather in the higher rate of progress exhibited by them' (192). 'Immature science' *i.e.* science that depends on theoretical considerations and world views is science in the full sense of the word (155f).

The book also contains a highly critical analysis of the sociology of knowledge and it concludes with a brief attempt to judge science as a whole.

2 RELATION TO OTHER VIEWS

To judge Laudan's theory of rationality we have to examine its relation to other philosophies and its effectiveness. Where and in what respect has Laudan changed and perhaps even transcended current views; how and to what extent have the changes improved the situation?

Laudan gives us extremely bad guidance on the first question. He emphasizes the importance of a comparative evaluation of research traditions but when he comes to his own model a shoddy account of the alternatives seems to suffice. His arguments against them often have the following interesting pattern: a philosopher (historian) is introduced as holding a view, or making a suggestion. *S* is examined, demolished and replaced by *Q* which is shown to be a natural consequence of Laudan's model. The model obviously is vastly better than its alternatives. Yet the poor philosopher never proposed *S*; he held *Q*, the very ideas Laudan presents as his own. On such occasions—and they occur rather frequently—Laudan sounds like a thief who chides his victims for lacking the items he has just taken from them. This is a very clever ruse and one would like to congratulate Laudan on it but unfortunately he has borrowed it from Lakatos:¹ Laudan's 'eclecti(cism)' (*ix*) is much greater than he is willing to admit. A few examples will show what I mean.

The general framework of Laudan's philosophy, *the problem solving model*, is of course well known (11). Anyone who has spent only a few days with Popperians and has tried to explain his ideas to them no doubt remembers the frustration caused by interruptions such as: what is your problem? You don't seem to have a problem—so, what are you talking about? I don't understand your problem, so there is no use going on with your story—and so on and so forth. A lover of Platonic imagery might describe the philosophy department at the LSE as a place where even the dogs no longer merely observe and react to the products of their fellow dogs but want to know the *problems* that made them produce such terrific solutions. Laudan's 'to write about the history of conceptual systems without ceaselessly identifying the problems which motivated those systems is drastically to misconstrue the nature of cognitive activity' (175) is a triviality for a Popperian as can be seen from the historical work that has emerged from that school.²

¹ See my [1975], p. 48, n. 2.

² See chapters 2 and 5 of Popper [1963]; Sabra's magnificent [1965] work on the history of optics; Lakatos's [1978]; as well as some of the case-studies in Howson (*ed.*): [1976] and Latais (*ed.*): [1976].

Problem solving can be combined with a variety of ideas about possible solutions and their evaluation. Within the Popperian circle we have the idea that solutions are proposed in the form of conjectures and are then criticised in accordance with standards which are themselves (temporary) results of a critical discussion. A theory or a research programme is judged by its problem solving capacity, *i.e.* on the basis of questions such as 'Does it solve the problem? Does it solve it better than other theories? Has it perhaps merely shifted the problem? Is the solution simple? Is it fruitful? Does it perhaps contradict . . . philosophical theories needed to solve other problems?' and so on (Popper [1963], p. 199). Note that these are precisely the questions raised by what Laudan calls his 'own' model (109), that they invite us to compare theories instead of trying to evaluate them absolutely and that they consider conceptual problems which according to Laudan, Popper (and Lakatos, and I) 'simply fail to come to terms with' (47). 'What we call "science"' writes Popper (*op. cit.* p. 127) 'is differentiated from the older myths not by being something different from a myth'—it does not cease to contain the 'conceptual' and 'world view' (61) assumptions characteristic of mythical thinking—'but by being accompanied by a second order tradition—that of critically discussing the myth': science is world views *etc. etc. plus* the problem solving model. Popper accordingly deals with conceptual problems of early science (*op. cit.*, chapter 5), he shows how the problem solving model treats non-empirical questions (metaphysical theories, though irrefutable, can be evaluated by comparing their problem solving capacities) (*op. cit.*, p. 199), he criticizes modern physicists for failing to take conceptual problems seriously (*op. cit.*, chapter 3 and various essays on the quantum theory) and himself proposes solutions for them.¹ It is true that Popperians have combined the problem solving model with truth, verisimilitude and corroboration and have more recently almost buried it under these ideas—but this does not impair the usefulness of the model itself for it can be discussed and developed 'without ever speaking about the truth of its theories'.² Laudan's '*no* major contemporary philosophy of science allows . . . for . . . conceptual problems' (66—his italics) is therefore somewhat inaccurate, to put it mildly.³

¹ For example, the propensity theory of probability.

² Popper, *op. cit.* p. 223.—Laudan repeats even details of Popper's view. Popper: 'the rational . . . character of science would vanish if it ceased to progress' p. 240; Laudan: 'rationality is parasitic upon progressiveness' (125). Popper: 'the growth of scientific knowledge may be said to be the growth of ordinary human knowledge *writ large*' (*ibid.*, 216 with reference to the Preface of the *Logik der Forschung*); Laudan: 'if there is any truth at all in the (positivistic) claim about the difference between the sciences and the non sciences . . . it will be found, not in the exclusive exhibition of progress by the sciences, but rather in the higher rate of progress exhibited by them' (192). And so on.

³ Laudan continues: 'even those philosophers who claim to take the actual evolution of science seriously (*e.g.* Lakatos, Kuhn, Feyerabend and Hanson) have made no serious concessions to the non empirical dimensions of scientific debate.' But Hanson like the good Wittgensteinian he was showed the strong influence of concepts on observation and experimental matters thus turning empirical problems into conceptual problems. Summing up a series of most interesting conceptual investigations he writes: '. . . we have tried to explore the geography of some dimly lit passages along which physicists have moved from surprising, anomalous data to a theory which might explain those data. We have discussed obstacles which litter these passages. They are rarely of a direct observational or experimental variety, but always reveal conceptual elements. . . .'

The same is true of Laudan's account of *research traditions*. He criticises Kuhn and Lakatos but what he finally comes up with is hardly distinguishable from their ideas. His criticism also shows an amazing inability to understand relatively simple historical arguments. Repeating familiar complaints he calls paradigms 'obscure and opaque' (74), 'difficult to characterise' (73), 'always implicit, never fully articulate' (75), he points out that 'Kuhn never really resolves the crucial question of the relationship between a paradigm and its constituent theories' (74) and that he does not indicate at what point anomalies are supposed to precipitate a crisis (74).

Now, first of all these complaints are *not correct*. The 'difficulties of characterization' have been overcome¹ and the question of crisis is answered by Kuhn himself who points out, in perfect anticipation of what Laudan has to say on the matter (18, 21, 29) that 'every problem that normal science sees as a puzzle can be seen, from another viewpoint, as a counterinstance and thus as a source of crisis'.²

But the complaints are *not reasonable* either. Rationalists assume a close correspondence between science and certain basic laws of abstract thought. Using

[1958], p. 157. Hanson's 1963 contains the following assertions: 1. 'The discovery of the positive electron was a discovery of three different particles' (p. 135); 2. there existed a 'profound resistance' against accepting a positively charged electron (159); 3. this resistance was due to the *conceptual* structure of 'electrodynamics and elementary particle theory' (159). Can there be a clearer refutation of Laudan's complaint? Kuhn discusses a great variety of conceptual problems, both in his book on the Copernican Revolution (esp. in chapter 4) and in his [1962] (67, 73f and *passim*). True, he makes special assumptions about the way in which conceptual problems are solved—they are developed until they generate empirical problems and then contribute to the anomalies of the underlying paradigm—but this does not mean that he disregards them or does not take them seriously (babies are taken seriously even by those who assert that they will eventually grow up). Lakatos has made us aware of long stretches of scientific development that are entirely conceptual and disregard empirical results ([1978a], p. 50) thus establishing the '*relative autonomy of theoretical science*' (*op. cit.*, 52—italics in the original) while his truly miraculous studies in the history of mathematics contain the best and most detailed presentation and analysis of conceptual problems in the entire history of ideas—there is nobody who has outdone him in this respect. (The objection that mathematics is not an empirical science and that Laudan's criticism applies to Lakatos's account of the empirical sciences only is removed by Laudan himself who praises Lakatos for having shown that 'even . . . the formal sciences' are full of empirical problems and therefore not essentially different from the empirical sciences—191). *I myself* have frequently been criticised for turning empirical problems into conceptual problems and thus robbing science of its empirical content and, indeed, most of my studies of the quantum theory, of classical mechanics (Brownian motion, for example), of the Copernican Revolution dealt with conceptual problems, problems of changing methods included. Finally, even a child is by now familiar with the way in which *logical empiricists* dwelt on consistency, ambiguity, circularity, *ad hocness* all of which are conceptual problems. Result: Laudan's criticism as presented at the beginning of this footnote fails already at the simplest task, *i.e.* the correct presentation of the views of those he had the 'good fortune' to meet 'as a student or colleague' (ix).

¹ Cf. the work of Sneed and Stegmüller as reported in section 4 of my [1977]. In sections 4 and 5 I also explain how the relatively stable parts of paradigms and research programmes can change and thereby refute Laudan's charge of 'rigidity' (75—against Kuhn; 78—against Lakatos). The charge is absurd in any case as the historical work of the Lakatos school shows.

² Kuhn [1962], p. 79.

the assumption they feel justified to demand an account of science that agrees with the laws and so they ask for clear definitions, full descriptions, unambiguous rules of procedure. Obscurity and opaqueness, indecision concerning the relation between basic entities (theories and paradigms, for example), lack of advice concerning the transition from anomaly to crisis are serious objections. They show that the analysis of science has stopped prematurely. Laudan wants a philosophy of science that is closer to the 'actual past of science' (158) and he also wants to separate eternal and specific parameters (130). This means that historical research and not rationalist declarations must now determine the nature of the entities used, their properties, their relations and their employment in the face of problems and that a *general* theory of science *must make room* for these specific parameters. It must leave specific questions unanswered and it must refrain from premature and research independent attempts to make concepts 'precise'. Kuhn's account perfectly agrees with these desiderata. His paradigms are 'obscure and opaque' not because he has failed in his analysis but because the articulation changes from case to case. The relation between theories and paradigms remains unresolved because each research tradition resolves it in its own way, in accordance with the cosmological, normative, empirical elements it contains. There is little specific advice concerning the treatment of anomalies because each paradigm deals with these matters in its own way. Laudan's accusation of incompleteness (which he takes over from a host of bewildered philosophers of science who have read a few logic books but have never seen science from nearby) shows that despite his severely historical posture he still shares the rationalists' dream for clear, well defined and history-independent conceptual schemes.¹

Laudan's accusation of implicitness, however, shows that he seems to be unaware of some very old debates concerning the difference between history and the physical sciences. Historians (and more recently, Wittgenstein) have pointed out that there are practices which proceed in a strict and regular manner but with only minimal explicit knowledge of the rules, laws and standards involved. We learn a language, including the many idiosyncracies it permits, we learn the ability to add to these idiosyncracies in the manner of poets (or thinkers)—but most of the rules that guide us are 'implicit and never fully articulated'. Learning a language or studying the regularities of a historical period does not mean studying rules in a rational manner, it means *immersing oneself* in a practice and being guided by an intuitive ability to imitate and improvise. Some older methodologists expressed this feature by saying that a historian studies a distant culture by trying to 'understand' it while a physicist who deals with explicit abstract notions 'explains'. Kuhn makes the highly interesting and revolutionary

¹ Laudan writes: 'Unless we can articulate workable criteria for choice between the larger units I call research traditions then we have neither a theory of scientific rationality nor a theory of progressive cognitive growth' (106). Precisely! And unless we can articulate workable proofs for the existence of God then we don't have a good theology either. But the question is whether there are such things as 'scientific rationality', 'progressive cognitive growth' and Gods. And to answer *this* question we must do some research using concepts that are not already adapted to the rationalists' dream and are therefore 'obscure and opaque' and 'difficult to characterize'.

suggestion that *physics is a historical tradition and therefore as much in need of Verstehen as history proper*. Laudan does not notice this feature of Kuhn's theory.¹

Next comes Laudan's discussion of *incommensurability*. As he tells it 'Kuhn, Hanson and Feyerabend began to despair about the possibility of any objective yardstick for comparing different theories and suggested that theories were incommensurable and thus not open to objective comparison' (193). This suggests that we wanted to compare theories, were misguided by some feature of science into believing that a comparative evaluation is impossible and joylessly published this disagreeable consequence. A look at our work reveals an entirely different story. What we 'discovered' and tried to show was that scientific discourse *which contains detailed and highly sophisticated discussions concerning the comparative advantages of paradigms* obeys laws and standards that have only little to do with the naive models philosophers of science have designed for that purpose. *There is* comparison, even 'objective'² comparison—but it is a much more complex and delicate procedure than is assumed by rationalists. Thus in my first paper on the matter³ I claim that 'a formal account of reduction and explanation is impossible for general theories' but show how predictions

¹ Summing up his account of research traditions Laudan enumerates a series of historical events and developments which according to him can be 'rationally justified' by the theory of research traditions but not by 'any other extant model of scientific growth and progress' (122f) and he concludes that 'the theory of research traditions . . . constitutes a significant improvement on the theories of rationality now in common parlance among philosophers'. But the events can be accounted for quite easily by Polanyi, Kuhn and *Against Method*. They can also be explained in Popper's two-tradition model (just try it, Larry, it is not at all difficult).

Incidentally, it should be pointed out that Laudan's distinction between 'trans-temporal *etc.*' and 'specific' parameters (130f) is old hat for Popper, Lakatos and even Kuhn. All these authors distinguish between paradigm-dependent standards and trans-paradigmatic standards (developments, in the case of Kuhn). They evaluate (describe) historical episodes by asking both to what extent they agree with the standards of the time and whether they are 'rational' (conform to the general pattern of development, in the case of Kuhn). With Popper the duality is part of his two-tradition model: every idea is subjected both to the standards of mythmaking (which change from time to time and place to place) and to the standards of critical discussion. Lakatos provides rich inventories of heuristic rules and standards including the 'normative images' Laudan is so concerned about (58, 164) side by side with his general criterion of progressiveness. Laudan's remark that Popper and Lakatos 'insist that we should evaluate historical episodes using *our* standards and simply ignoring the appraisals made by the relevant scientists about the rationality of what they were doing' (129) is just another instance of the great gulf between his account and the actual views of the people he criticizes.

² None of the writers who defend 'objective' standards has explained what the word means. Laudan uses the word to *criticise* but again without explaining what lack of objectivity amounts to and why it should be feared. Popperians occasionally connect objectivity with truth (in Tarski's sense) and call comparisons 'objective' only if they are based on a comparison of truth content. Incommensurability rules out such a comparison. For a Popperian the remaining standards (*and there are lots of standards left*) are 'subjective' which is the reason why I call them 'subjective' in my criticism of Popperians in my [1970], p. 227. Laudan takes the passage as indicating that *I myself* hold them to be 'subjective' (letter of June 17, 1976) and he assumes that I apply incommensurability to *all* means of comparison, not only to means that depend on content. But already the next few lines of my [1970] tell a very different story.

³ [1962], see p. 28.

can still be used for comparing theories:¹ what fails is not the process of theory comparison, what fails is a rather simpleminded theory of explanation. According to Kuhn 'to say that resistance (to paradigm change) is inevitable and legitimate, that paradigm change cannot be justified by proof, is not to say that no arguments are relevant or that scientists cannot change their minds.'² 'Probably the single most prevalent claim advanced by the proponents of a new paradigm is *that they can solve the problems* that have led the old one to a crisis'.³ Compare this with Laudan's '. . . an approximate determination of the effectiveness of a research tradition can be made *within* the research tradition itself . . . we simply ask whether a research tradition has solved the problems which it set for itself' (145f) read in conjunction with the assertion that paradigms may have 'joint problems which can be formulated so as to presuppose nothing which is syntactically dependent upon the specific research traditions being compared' (144) and you will see that except for Laudan's longwindedness there is not the slightest difference between Laudan and Kuhn. But Laudan presents his *repetition* of Kuhn as a suggestion designed to remedy a *flaw* in Kuhn's account—which is precisely the pattern I have described at the beginning of the present section.⁴ There is absolutely nothing Laudan can tell us about theory comparison and theory evaluation.

Which brings me to the last item on my list. Laudan not only criticizes philosophers and historians for having neglected important features of science, he also takes them to task for their 'flagrant disregard for the actual past of science' (158) which, according to him 'is deeply grounded in their convictions about the aims of a philosophically based history of science' (168). And he criticises especially Lakatos for 'consciously and deliberately falsifying the historical record' (170). Now while I don't know how 'conscious' Laudan himself was when telling his fairytales about Popper, Hanson, Kuhn, Lakatos and the humble author of the present review and how 'deeply grounded' his fabrications are in his wish to appear original, I am certainly amazed at the difference between these fabrications and the 'historical record'. The accusation just quoted is another instance of this pattern. Take Lakatos. He writes:⁵ 'In writing a historical case study one should, I think, adopt the following procedure: (1) one gives a rational reconstruction; (2) one tries to compare this rational reconstruction with actual history and to criticise both one's rational reconstruction for lack of historicity and the actual history for lack of rationality. Thus any historical study must be preceded by a heuristic study . . .' He illustrates the principle

¹ My [1962] p. 94. In my [1958] (two years before I saw the MS of Kuhn's book and four years before the book appeared), 163f I 'consider' the 'objection' that basing interpretations on theories 'makes nonsense of crucial experiments' and show how we can still use them. I criticise *philosophical interpretations* of crucial experiments, I do *not* criticise the *practice*.

² Kuhn [1962], p. 151.

³ *Op. cit.*, p. 152; my italics.

⁴ Thus Laudan (145f), partly using Kordig, proudly present procedures that survive incommensurability, and implies that none of us ever thought of such a clever escape. But my [1970], p. 222ff discusses *exactly the same* procedures, and in greater detail than Laudan while Hanson, in his magnificent analysis of the correspondence principle showed long ago how incommensurable theories can be compared and so made an important contribution to our understanding of research amidst changing ontologies: [1958], p. 148f.

⁵ *Op. cit.*, p. 52—italics in the original.

partly with sketches whose 'caricature'—character he explicitly emphasises,¹ partly with detailed studies such as his incomparable *Proofs and Refutations* where the reconstruction is presented in the form of a debate while 'the real history . . . chime(s) in the footnotes, most of which are to be taken, therefore, as an organic part of the story'.² Where is the 'conscious and deliberate falsification'? Lakatos's reconstructions are *blueprints* which he presents *in addition* to the buildings whose structure they are supposed to determine. Nobody would call a blueprint a 'falsification' because the builders chose to disregard it.³ At any rate—the procedure is very different from Laudan's who introduces his account without qualifications, as if it were already the Real Thing.⁴

Laudan's description of the present situation in the philosophy of science is an extreme example of a widespread phenomenon: every profession has a body of beliefs which are hardly ever examined, are out of touch with reality and yet play an important part in arguments and the associated propaganda. Examples are the assumption of the empirical nature of the Copernican Revolution, the assumption that Newton derived the law of gravitation from facts, that Boltzmann was an old-fashioned realist fighting valiantly against positivists deadbeats such as Mach and Ostwald, that Marxists live off *ad hoc* hypotheses, that Einstein took falsifications seriously, that astrology has no connection with reality, that every illness proceeds from a localisable material process. Laudan's book, despite its belligerently historical stance shows some fairytales *in statu nascendi* and its reception shows how quickly and readily philosophic folklore accepts them.⁵ I have tried to restore—not too successfully, I am sure, at least part of the real story. What remains? Popper's original problem solving model freed from the cumbersome logical machinery Popper himself and some of his more distant pupils have superimposed on it,⁶ supplemented with a pinch of Kuhn and seasoned with generous helpings from the work of Laudan's other victims. Let us now see how this model fares when compared with science, commonsense and itself!

¹ *Ibid.* p. 55 fn. 3.

² Lakatos [1978], p. 5.

This also answers McMullin's criticism mentioned by Laudan (168, fn. 17).

To show my shortcomings Laudan refers to papers by McEvoy and Machamer (168, fn. 17). But McEvoy cannot be taken seriously (*cf.* my [1978] 160 and fn. 17) and Machamer's history, though more bulky, is hardly better than Laudan's. Besides, I have replied to him and refuted his criticism point by point (see the reprint in my [1975], 112f). Laudan wisely, though somewhat deviously neglects to mention this reply even though it was published in his own journal.

⁵ 'Ringing in the New' is the title of two reviews, including Burian's review of Laudan's book: *Isis* 69 (1978), 602.

⁶ It is interesting to see that fundamental discoveries which show the limitations of simpleminded modes of thought are as a rule succeeded by the belligerent reaffirmation of these modes. Popper criticised the formalist character of the Vienna-Circle philosophy but he soon introduced technicalities of his own (corroboration; verisimilitude) which for his less gifted successors have become the Alpha and Omega of rationalism. Today the problem is no longer the advancement of science but the preservation of a school philosophy. Ordinary Language philosophers once laughed at the childish pretensions of formal logic only to fall for them in the end. Imre Lakatos gave splendid examples of the looseness of proof patterns in informal mathematical logic only to prefer a more rigorous logic towards the end of his life (*cf.* the editorial comments on page 138, fn. 4* and page 146, fn. 2* of his [1978].) Even the skeptics did not escape this pattern: the informality of Pyrrho was soon followed by the learned discourses of Carneades and completely killed in the presentation of Sextus Empiricus.

3 ADEQUACY OF THE MODEL

Laudan makes a point of asserting that his model, while vastly more liberal than its competitors, is not without limits. 'To suggest that "anything goes", that any combination of beliefs would emerge as rational and progressive on this model is profoundly to misunderstand the high standards of rational behaviour which it requires' (128). Moreover, 'for scientists in any culture to espouse a research tradition or a theory which is less adequate than other ones available *within* that culture is to behave irrationally' (130), (adequacy is defined as problem solving capacity—109, 124). It is also irrational to *argue* against theories or research traditions on the basis of non-progressive world views (132). Finally, Laudan would presumably regard it as irrational to *pursue* inadequate research traditions of a (comparatively) low or even a negative ratio of progress (*cf.* 111). His model does indeed impose limits. But the question is: are the limits important, are they realistic, can they be upheld within the model? It seems that the answer to all three questions is negative.

To start with, let us point out that a model may have content in the sense that it forbids actions and calls them irrational but may still be practically vacuous in the sense that the forbidden actions are of no interest to anyone. Lakatos, for example, does not permit people to *call* degenerating research programmes progressive—*this is the only 'rational' weapon he produces against 'chaos'*—but who is going to start a war over such a trivial matter?¹ Laudan's objection to 'espousing' inadequate theories, though less obviously trivial, has similar cash value: inadequate theories may be 'pursued', they may not be 'accepted' (108ff). What does it mean to 'accept' a theory? 'To treat it as if it were true' (108—italics in the original). According to Laudan the relation of theories to truth plays no role in science. So it cannot be used to separate acceptance from pursuit. What remains is the order *not to say* that one has 'accepted' a theory that is comparatively inadequate and yet plays a role but to speak of 'pursuit' instead. Big deal! The appeal to commitment (a scientist who has accepted a theory 'must commit himself'—109) does not help. First, because according to Laudan commitment may be tentative to a high degree ('however tentatively' 109)—and, secondly, because there exists no behavioural difference between commitment and vigorous pursuit: if there are various paths open for reaching an aim and you are not sure which path to choose you may start walking along the first path which is exactly what you would be doing if the path were the chosen one. But assume we admit that not all theories are treated in the same way: some medicines are fed to rats while others are released for human consumption. Then the

¹ For the background of this criticism *cf.* my 'On the Critique of Scientific Reason' in Howson (*ed.*): [1976], esp. 324, end of footnote 45. Musgrave (Radnitzky-Anderson (*eds.*) [1979], p. 192) objects: 'Lakatos is no *epistemological* anarchist since he provides a whole battery of standards for judging theories and research-programmes.' Now first of all it is not Lakatos who 'provides' these standards, but scientists: none of the standards which Musgrave mentions further down on the page were invented by Lakatos. He took them from history. Secondly, scientists use these standards *opportunistically*—they sometimes follow them, then again they pay no attention. Lakatos condones this opportunism pointing out that it is backed and held together by his general theory of rationality. But this general theory does not work—even Musgrave admits this now. What remains are lots of rules guided by a healthy opportunism that changes from one case to the next. In a word—what remains is *anything goes*.

difficulty is that this difference does not help us with purely theoretical problems; nor is it clear that we are dealing with a difference between acceptance and pursuit rather than a difference between different forms of pursuit. Some pharmacologists may of course say 'this is it!' and stop looking for side effects—but it would be more than a little absurd to honour such an attitude by creating a special epistemological category; and if the category exists despite its absurdity then it is wise, in the interest of human welfare, to stay on the side of pursuit and to warn patients of doctors who have moved over to acceptance. I conclude that the distinction between acceptance and pursuit may characterise *special cases* but it would be either vacuous or, if not vacuous, unwise to make it a basis of general rules for the evaluation of research traditions.

The conditions of pursuit, on the other hand, are much too restrictive. Laudan distinguishes between the *adequacy* of a theory—it has *solved* more problems than its competitors—and its *promise* which is the *rate of progress* in solving problems. A highly inadequate theory may be promising in this sense (112f) and deserve being pursued while inadequacy and lack of promise speak against it: a theory, a research tradition, a world view *must perform well* before it can become part of research. But how can we judge its performance if we have not already made it part of research? To object to the pursuit of some idea unless there is some guarantee in terms of performance means putting the cart before the horse for the required guarantee can be obtained only by means of the very same research one wants the guarantee for.¹ And indeed we find, when looking at history, that lack of performance and inadequacy have never stopped people from pursuing views they regarded as important. Atomism, Platonism, the idea that the earth moves, the idea that the laws of nature have a history, relational accounts of space and time were proposed or revived not because they had performed so well in the past but because one believed them to possess an (as yet unrealised) *ability* to perform. The many revivals of Platonism, atomism, of magical world views, the rise of rationalism in Greece can hardly be explained in Laudanian terms, even if we consider conceptual matters only. These phenomena have much in common with revivals of *faith*—but they contributed to the advancement of science. The same faith in potential rather than in actual performance, or 'promise' in Laudan's sense was responsible for some of the most interesting developments in the history of thought. At the beginning of Western rationalism abstract argument was faced by almost unsurmountable problems (paradoxes of Parmenides and Zeno; difficulties in mathematics; problem of relation between commonsense and philosophical theory, city law and philosophical law, perception and 'reality'—all of which were noticed and discussed in the works of Plato and Aristotle). Some of the problems were 'solved' (very often in an *ad hoc* manner, by turning them into principles) and the solution created more problems. This delights critical rationalists but is a difficulty for Laudan: a research tradition which in solving problems proliferates problems has a negative rate of progress; yet one continued on the troubled path and used it to change existing

¹ The fact that Lakatos and Laudan introduce rules of pursuit shows that they have not understood Popper's (or, rather, Mill's) anti-inductivism (I am now thinking of Mill's *On Liberty*): justification *comes with research*, it cannot be a *precondition* of it; nor can one expect it to turn up within a well defined time interval, as a result of steps that are known in advance.

traditions which were adequate and perhaps even progressive. The 'Copernican Revolution' is another instance of the principle, denied by Laudan, that in intellectual matters it is expectation, faith, hope or simply ignorance (of problems) and not actual performance that explains pursuit. In the *Commetariolus* Copernicus criticised astronomy for its reliance on the equant. The principle of his criticism was the idea that real astronomical motions are centred circular motions with constant angular velocity and that an explanation of phenomena must consist in their reduction to such motions. In 1520/40 the principle was neither adequate nor progressive in Laudan's sense while the equant was at least adequate. Yet the problems created by the conflict between the principle and Ptolemaic astronomy were taken seriously enough by Copernicus, the Wittenberg astronomers (as reported by Westman), Brahe to justify attempts at rebuilding astronomy: theories or world views are permitted to create problems even though they are neither progressive nor adequate. Moreover, the Copernican arrangement was itself beset by problems. The problems are disregarded by philosophers of science who restrict themselves to astronomy. But expanding the domain of discussion into physics, optics, theology as Laudan suggests and counting successes *as well as failures* (78, item 5—against Lakatos) the rate of progress is considerably decreased. In the 17th century the situation becomes more opaque, but not better. For the new philosophies that are now introduced to accommodate the new cosmos create further problems such as the mind-body problem (as yet unsolved), the problem of the relation between God and the world, the Word of God and the Work of God (taken very seriously by Newton), the problem of motion (in the Aristotelian sense, including qualitative change) which are fundamental, have greater weight than technical problems and which have resisted solution to the present day. The elimination of witchcraft theories, to mention only one side effect of the development left a great variety of psychological problems unsolved without having anything better to offer—and this situation lasted until the 19th century. *Nobody knows* what the *overall rate of progress* was, nobody knew it then and *nobody cared* partly because the difficulties were not noticed by those 'at the forefront of research' (ignorance), partly because they were not regarded as important, partly because special achievements in a narrow domain were regarded as sufficient reasons for carrying on at all fronts (for the period in question Lakatos's emphasis on success over failure—criticised by Laudan, 78—is on the right track) but mainly because potential was more important than promise in the sense of Laudan (on this last point Lakatos and Zahar again seem to be more clear-sighted: they emphasize progressiveness but refrain from turning it into a principle of pursuit; and they make an analysis of the *heuristic promise* of a research programme an essential part of its evaluation).

I have said that world views are often used in arguments against successful traditions even though they are neither adequate nor progressive and I have mentioned 19th century atomism, ancient Greek rationalism and Copernicus's principle of circular motion as examples. Actually, the situation is much more complex. Principles are never used in isolation, but in conjunction with other principles. Some new idea that clashes with established results and gives rise to numerous conceptual and empirical problems often gains strength from other ideas that are equally inadequate and unpromising (in Laudan's sense) but sup-

port it and gain support from its further articulation. Everybody who has taken the trouble to study philosophical systems such as those of Hegel, or Aristotle has not doubt felt the intellectual force emanating from the collaboration of ideas which, taken one by one, are implausible, unrealistic, in conflict with their surroundings.¹ In this way Parmenides's ideas drew strength from their internal coherence though they clashed with everything around them, progressive practices included. And this is also the way in which ancient ideas such as the atomic theory survived through the ages and finally overcame their most successful rivals.

To sum up: inadequate and unpromising research traditions, word views, theories *are* often used as parts of research; they *must be* used in this way or else their virtues will never be revealed and they *are aided* in their use by the mutual support of the ideas they contain. Faith, good sense and internal articulation collaborate in a procedure which, according to Laudan, is 'patently absurd' (132). Moreover—and with this we come to a further objection against him—such use may considerably reduce the success of the theories criticised and with it the absurdity of the procedure. As Laudan presents the matter world views, theories, research traditions are either progressive or not progressive. Only in the first case are they permitted to judge and to criticise. How do we find that a world view is non-progressive? By showing that it has many problems but has neither solved them nor shown promise to solve them. How do the problems arise? From conflicts with research traditions, facts and so on. Such conflicts can be turned against the world view, *but they can also be turned against the research traditions*. In the first case we have a world view with problems, in the second case with have a research tradition with problems. According to Laudan the direction in which the conflict is turned depends on the past history of the world view: if many problems got solved in accordance with its principles, then the conflict can be turned against the research traditions. Otherwise the world view is disregarded. We have already seen that this is an unreasonable step: it would forever exclude new world views from entering the scene. If we want to try out such views, then we must be prepared to take the 'patently absurd' step and must turn them against successful traditions even before they have achieved their own first success. We must permit them to create problems for these traditions and challenge the defenders of the traditions to solve the problems. We have also seen that this is indeed the way in which revolutionary changes are brought about—but now we have a new problem: how do world views which have received the power of life and death over research traditions ever lose this power? Or to put it differently—how does it happen that the problems they create are taken less and less seriously and are finally not regarded as problems at all? For example, how is it that the problems a substantial Christianity created for the mechanical world view (and which Newton took very seriously) are no longer with us? Because the mechanism that guides the exchange of world views *constitutes* what counts as a problem or as a good performance and is therefore relatively independent of performance and problems. What is this mechanism?

¹ Duhem has gone very far in reconciling a basic Aristotelianism with the methods of modern science (Simplicius and Aristotle himself preceded him in this respect). And Professor Kuhn once told me that seeing the internal coherence of the Aristotelian philosophy and its ability to deal with problems in its own terms was an important step towards the theory of paradigms. Cf. his [1977], p. xi ff.

Change of allegiance from one world view to another world view or another research tradition. *Change of allegiance i.e. a socio-psychological process is primary, calculation of performance, 'rationality' comes after it and depends on it.* So, we are finally back at our earlier result *viz.* that (excepting special circumstances) *performance has no direct influence in what views are accepted and what views rejected* and it cannot have such an influence, from the nature of the case. Reason, however, turns out to be a locally acting agency whose application and whose limitations depend on circumstances of an entirely different kind.

There is still one element missing from my evaluation of Laudan's version of Popper's model: the model has many ways of *circumventing* rules without violating them. There arises therefore the suspicion that a determined application of all the methods it offers can overcome difficulties by reducing content in precisely the manner in which Lakatos's model, in trying to survive attacks, is finally pushed into excluding trivialities only. The suspicion is confirmed by noticing that Laudan can accommodate empirical and conceptual problems with the help of *ad hoc* hypotheses (115), or by denying that the theories that give rise to conceptual problems make substantive assertions (instrumentalism), or that their observations are *bona fide* observations (devaluation of observations by declaring them to be illusions). It can reduce rates of progress by denying the existence of problems already solved and increase them by either changing from one methodology to another (permitted by Laudan—59) or from one world view to another, or else by restricting research programmes to domains where they show success and making the success achieved there a measure of their overall progressiveness. All these procedures have occurred in the history of science, they have advanced (in our sense, or in the sense of the users) science at decisive periods and they can therefore be supported by pointing to the 'actual development of science'. But the trouble is that Laudan, in making them part of a *unified* theory of rationality with *general* rules like those quoted at the beginning of the present section will be inclined to use them where they do not help or void them of content. The trouble is that, like Lakatos before him, Laudan gives us a model that is either inadequate or trivial.

Now rationality was not always in this predicament. When it arose in Greece it created mathematics, astronomy (in our sense) the history of ideas, biology, psychology, the theory and practice of drama, theology and many other subjects. All these subjects were created as the result of procedures which one might call rational. But the *study* of knowledge soon got separated from the processes that *created* it. While Aristotle examined (the conditions of) existing knowledge and created new knowledge in accordance with the results if his examination his successors, among them Kant, Mill, Whewell were content to explore the structure of an already existing body of knowledge without adding to it. In the 20th century attention was concentrated on the instruments of the exploration. Logical sophistication increased, but the ongoing process of (scientific) research became more remote than ever. Lakatos and Laudan are the late children of this development. Once, long ago, lady Reason was a beautiful, strong, helpful though somewhat overbearing Goddess of research. By now her lovers (or, should I rather say, pimps?) have turned her into a garrulous but toothless old woman.

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KUHN AND THE QUANTUM CONTROVERSY*

- 1 Introduction
- 2 Planck's Work before 1900
- 3 Classical Physics or Quantum Physics?
- 4 Summary and Conclusions

I INTRODUCTION

One of the major publishing events of the year in the history and philosophy of science has occurred with the recent appearance of Kuhn's book. The immediate interest it has aroused is reflected in the special symposium *Isis* (vol. 70, 1979) has just published including reviews by Martin J. Klein, Abner Shimony and Trevor Pinch. The backgrounds of these participants in history, philosophy and sociology suggest the wide range of audience that follow Kuhn's work. Indeed, for historians of physics, Kuhn's latest book represents a major and already controversial reassessment of the introduction of the quantum into physics. As such it touches on an issue of singular importance, one whose resolution will influence our whole account of early quantum theory including the roles of Planck, Ehrenfest, Lorentz, Einstein, Sommerfeld and Bohr in setting out the new physics. Philosophers will want to know what light this study throws on

* Review of THOMAS S. KUHN [1978]: *Black-Body Theory and the Quantum Discontinuity, 1894-1912*. Oxford: Clarendon Press. £12.50. Pp. 356. Page references without date below are to this book.