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REVIEW ARTICLE

The Value of a Fixed Methodology*

I INTRODUCTION: SCIENTIFIC CHANGE AND ITS PROBLEMS

Larry Laudan's latest book focusses on disagreements in science and on how such disagreements are resolved. Very briefly, Laudan holds that there is more disagreement in science then can be coped with by the accounts of scientific change given by such philosophers as the logical positivists, Reichenbach, Hempel and Popper (and by such sociologists of science as Merton); but that the fact that such disagreements are 'often' 'definitively' resolved is incompatible with the newer 'holist' or 'big picture' view associated with Kuhn and others. Laudan therefore sets out to develop his own explanatory model of scientific change which will allow *both* for the possibility of wide-ranging (and rational) disagreement *and* will explain how such disagreement might eventually be definitively resolved.

He sees the recent history of science studies in something like the following terms. The 40s and 50s were dominated by the 'consensual view'. On this view scientists standardly agree (about more or less everything and more or less all the time)—disagreements about theories *do* occasionally break out, but are quickly resolved by appeal to shared methods and goals. Then came Kuhn. His principal role was to overturn the consensual view by showing that disagreement is endemic in the scientific enterprise: scientists' disagreements about factual matters are standardly deeper and longer lasting than had generally been supposed and, more importantly, they need not be readily resolvable, because disagreement often extends beyond the factual to the *methodological* level, and even to the level of the appropriate aims and goals for science. Kuhn thus ushered in the '"new wave" preoccupation with dissensus'.

* Laudan, L. [1984]: *Science and Values. The Aims of Science and their Role in Scientific Debate.* University of California Press. Pp. xiv + 149. US $14.95.

An explanatory 'model of consensus formation' developed alongside the consensual view: the 'hierarchical model' (which Laudan associates with such philosophers as Reichenbach, Popper and Hempel). This model identifies three levels of scientific commitment—factual, methodological and 'axiological'. It assumes that methodology governs factual matters and axiology governs methodology. Factual disagreements (understood, of course, in a broad sense which includes theoretical disagreements) are standardly resolved by appeal to shared methodological standards: if two scientists disagree about which is the better of two theories, they will quickly restore agreement by consulting their *shared* appraisal criteria which will rank the two theories for them. However, Laudan sees it as one of the important truths learned from Kuhn that disagreements between scientists can extend beyond the substantive level to the level of methodology (p. 25). Such disagreements need not embarrass the hierarchical model—since appeal can be made to the axiological level, where shared goals and aims may decide the methodological disagreement (ibid and p26). The hierarchical model *is*, however, in trouble, if disagreement extends to the top 'axiological' level, and it *does*:

> The history of science is rife with controversies between, for instance, realists and instrumentalists, reductionists and antireductionists, advocates and critics of simplicity, proponents of teleology and advocates of purely efficient causality. At bottom, all these debates have turned on divergent views about the attributes our theories should possess (and thus about the aims of scientific theorizing). (p. 42)

A rival to the hierarchical model of scientific change developed alongside the 'new wave preoccupation with dissensus': the 'big picture' or 'holist' model. This has scientists disagreeing at all three levels and indeed it sees change as occurring at all three levels simultaneously. 'Paradigm shifts' involve not only changes in theoretical commitment, they also involve changes in ideas about how to appraise theories and even in ideas about the appropriate aims for science. But then nothing is left fixed to act as neutral arbiter between two successive paradigms. This means that, while the 'holist' model can certainly accommodate widespread 'dissensus', it cannot *explain* 'how—short of sheer exhaustion or political manipulation—scientific disagreements are ever brought to closure' (p. 16). Yet Laudan sees it as a fact that such disagreements are 'often' and 'definitively' resolved.

On Laudan's account, then, the development of post-war science studies has resulted in a fundamental problem. There is too much disagreement in science for the 'hierarchical model' to handle; yet more often than not such disagreement is (at any rate *eventually*) resolved in apparently rational fashion and this is inconsistent with the 'big picture' model—the only alternative model so far available. The main aim of Laudan's book is to develop a third model which delivers the best of both worlds.

Laudan's interesting attempt fails. In section 2 I argue that Laudan's 'reticulated model' is not a genuine third alternative. And in section 3 I argue that Laudan has both seriously misidentified the older, 'hierarchical view', and seriously overestimated the impact on it of Kuhnian criticisms. My conclusion is that the 'older' approach aimed at laying down *fixed* principles of scientific theory-appraisal is the only alternative to relativism.

2 THE 'RETICULATED MODEL' AND ITS FAILURE

Laudan's reticulated model retains the three levels of its predecessors. Like the holist model, it allows for changes at all three levels, but it insists that the holist idea of *simultaneous* change at all three levels is historically quite inaccurate. Like the hierarchical model, it sees shared methodological rules as justifying certain theory-choices, but it insists that the hierarchical idea of *one-way* justification is quite wrong. Instead, accepted theories may also 'constrain' methods and accepted methods may dictate changes in presumed aims and goals. Thus Laudan's reticulated model involves

> a complex process of mutual adjustment and mutual justification going on among all three levels of scientific commitment. Justification flows upward as well as downward in the hierarchy, linking aims, methods, and factual claims. No longer should we regard any one of these levels as privileged or primary or more fundamental than the others. Axiology, methodology and factual claims are inevitably intertwined in relations of mutual dependency. (pp. 62–3).

Suppose, then, that the holist points to an allegedly 'revolutionary' change in 'world view' from one theory-methods-aims triad (T_1, M_1, A_1) to another (T_2, M_2, A_2). Assume that (T_1, M_1, A_1) was indeed accepted by the 'scientific community' at one stage and that (T_2, M_2, A_2) was accepted at a later stage. The holist sees the change as occurring all at once—thus precluding rational explanation. The reticulated modeller has at his disposal several possible 'rational paths' from one to the other. For example,[1] T_2 may come to be preferred to T_1 while M_1 and A_1 are still in force; M_2 is then proposed as possibly superior to M_1; the decision as to whether M_2 is indeed preferrable to M_1 will generally involve both A_1 and T_2 (it is a question of whether M_1 or M_2 is 'optimal for securing A_1' and this is 'typically . . . an empirical matter', which involves our best theory—in this case T_2). Assume that M_2 emerges victorious; it may later turn out 'that virtually none of the theories accepted by the scientific community as instances of good science', presumably including T_2, 'exemplify the values expressed in A_1'; and this may require (and supply the rationale for) a switch to A_2.

'Feedback', 'mutual dependency', 'upward and downward' justification all

[1] This is Laudan's 'tell tale' of paradigm change, sketched around pp 76–8. The same failing I point to in the text surely afflicts his other schematic cases too: a theory change cannot at the same time be explained as rational by a methodology and explain as rationed a *change* in methodology.

leading to the 'rationalisation' of change: Laudan's 'reticulated model' sounds just the ticket. Unfortunately it is a ticket onto the rocks.

Assume for the moment that the methodologies involved here, that is, M_1 and M_2, are the norms which *really* govern the preferences of the scientists concerned. (As Lakatos forcefully argued such '*implicit* methodologies' might differ radically from the norms which the scientists *explicitly* articulate and defend when indulging in published methodological reflection.) Next assume with Laudan that theory T_2 is accepted over T_1 while M_1 is in force. I presume that this implies that M_1 really does—'objectively'—rank T_2 more highly than T_1. (If not then Laudan's reticulated account immediately and clearly breaks down as an *explanation* of change.) I also take it that T_1 and T_2 are supposed to be the only serious rivals around at the time. It follows that M_1 pronounces T_2 the (temporarily) best available theory.

But how, then, could T_2 possibly 'constrain' a change in M_1, as Laudan's second step requires? M_1 has done all that T_2 could ask of it: ranked it top of the available theories. If the acceptance of theory T_2 really required the switch to methodology M_2 then T_2's initial acceptance, *while the earlier methodology M_1 was in force*, could not have been rational.

The problem can be seen very sharply in Laudan's favourite historical example. He sees the early 19th Century revolution in optics as having involved radical changes not only at the theoretical, but also at the methodological, level. Not only the Newtonian theory of light, but also Newtonian inductivist methodology, was predominant in the 18th Century. This methodology—on Laudan's account—banned all genuinely theoretical entities and put no premium on *predictive* success. The revolution saw not only the acceptance of the wave theory of light but also the acceptance of a more liberal 'hypothetico-deductive' methodology which sanctioned theoretical entities and stressed the probative value of novel prediction. In earlier accounts Laudan was quite explicit: the methodological change was *real* not just *professed*, and it was brought about by the theoretical change—a theory which gives a central role to the space-filling but invisible and intangible 'luminiferous ether' could hardly co-habit for long with a methodology which anathematised theoretical entities.

But, aside from any question of the historical accuracy of this account, it faces a clear (and devastating) logical problem: if Newtonian inductivism really were in force at the time Fresnel developed his theory (and *if* Newtonian inductivism really does ban genuinely theoretical entities) then the acceptance of that theory by the scientific community could not have been rational. Conversely, of course, if the initial acceptance of the wave theory was rational, then Newtonian inductivism (as described by Laudan) was not really in force at the time. In neither case can there have been a real shift in methodology which can be explained as the rational response to the prior acceptance of the wave theory.

Some remarks suggest that Laudan may now have moved towards an account of this episode more in terms of 'explicit methodology'. It is of course perfectly possible to construct a logically flawless account of this episode in these terms. This would go as follows. Fresnel's theory was *intuitively* accepted as enormously successful—as really superior to the Newtonian corpuscular theory on scientists' real, *implicit* standards. However, scientists in the 18th and early 19th Centuries had been spoilt by the success of Newtonian physics, were under the *illusion* that that success had been achieved by applying the strictly positivistic Newtonian method and hence tended, in their *explicit* methodological writings, to emphasise induction and 'deduction from the phenomena' as the correct scientific procedures. Their intuitively high appraisal of the wave theory, coupled with the glaringly obvious fact that the 'luminiferous ether' is a highly theoretical entity, helped free them from this (explicit) methodological illusion, and helped them to see that 'deduction from the phenomena' is a myth (that is, a myth at the level of *real*, 'implicit' methodology). On this *second* account, the 'methodological revolution' involved no *real* change in methodology, but consisted in scientists' explicit methodological pronouncements coming much more closely into line with the implicit methodological standards they had in fact always applied.

The problem with this second account from Laudan's point of view is that it provides no example of 'reticulation'. On this account there was no change at the real, 'implicit' methodological level. Indeed it is the (presumed) fact that Fresnel's theory was superior to its predecessor according to *unchanging* implicit methodological standards which explains the shift to that theory as rational.

Laudan is in a dilemma. If the methodological change that he claims has occurred is change in implicit methodology, then his 'reticulated' account supplies no rationale for scientific change. In this case in fact his 'reticulated model' collapses into 'big-picture relativism'. If, on the other hand, those methodological changes are only in 'explicit' methodology, then they present no challenge to the 'hierarchical model' (which, at any rate as I understand it, is concerned solely with real, implicit methodology). In neither case does the 'reticulated model' stand as a genuine third alternative.

This conclusion is cold comfort if the 'hierarchical model' of change is in as serious trouble as Laudan alleges. For that would leave 'big-picture relativism' as the only alternative—and an abhorrent alternative at that. Fortunately, as I shall argue in the next section, Laudan both misdescribes the 'hierarchical view' and greatly exaggerates the impact on it of Kuhnian criticisms.

3 THE 'HIERARCHICAL MODEL' REVISITED

Laudan takes it that Kuhn and others have demonstrated that there have been important changes at the methodological level in science, and at the level of aims and goals. These changes cannot be accommodated within the 'hierarchical' view. But if we are to decide whether or not there has been methodological change in the history of science, we clearly need first to decide exactly what counts as a methodological principle. And ordinary usage is far from unambiguous on this point. It seems to me that Kuhn (and Laudan) take a *much wider* understanding of methodology than the earlier 'positivist' philosophers: to the extent that *none* of Kuhn's and Laudan's examples of alleged methodological change are changes in methodology as understood by those earlier people. This means that, despite the rhetoric, this older view remains, in this respect at least, totally unaffected by these Kuhnian criticisms. Since it need not accept that there has been any methodological change *in its sense*, the hierarchical view need not bring in aims and goals to arbitrate such changes, as Laudan suggests it will. This is fortunate since the 'axiological' level would be quite unequal to this task. Let me expand on these compressed remarks.

(i) *The 'older view' of methodology*

Just as scientists do not usually indulge in meta-science but instead get on with their scientific work, so methodologists do not usually indulge in meta-methodology. Nonetheless I take it that Carnap, Hempel, Reichenbach as well as earlier thinkers such as the French conventionalists Duhem and Poincaré, all thought of themselves as aiming to articulate the principles which *invariably* govern the appraisal of scientific theories. The idea was to do for the 'logic of science' what Boole, Frege, Russell and the mathematical logicians had done for *deductive* logic: just as the latter had articulated the general and unchanging principles of valid inference, so those methodologists were trying to find the general and unchanging principles of 'inductive logic', and in particular of when one theory was, in view of all the evidence, better supported than another. Of course, just as inferences are sometimes made which are invalid, scientific appraisals may sometimes be made which are incorrect. It is even possible that whole historical epochs may be characterised by consistently 'incorrect' appraisals; and it is certainly possible that attempts to *articulate* these principles may go badly astray. But just as the existence of invalid arguments does not challenge the abstract idea of deductive validity and certainly does not imply that the objective standards of validity have changed over time, so the existence of incorrect scientific appraisals does not challenge the idea that—at the abstract, logical level—there are fixed principles of theory appraisal, and it certainly does not imply that the real standards of appraisal have changed over time.

In order to play the envisaged role, the principles of scientific theory appraisal were clearly going to need to be principles of great generality and abstractness, relying on no controversial 'metaphysical' assumptions about the structure of the universe. There were of course (and continue to be) hotly debated differences over the correct exact articulation of these general rules. But I take it that there was no suggestion that two different sets of principles might be the right ones for different sets of scientists at different times. Indeed there was a great measure of agreement between the different philosophers *at the intuitive level*—agreement that the crucial question is how well the different theories stand up to the empirical evidence. The differences tended to be over such further questions as whether or not the shared intuitive judgments about empirical support were captured by some formal, probabilistic system. On this 'older' conception, then, even the best scientists might, on occasion, make a methodological mistake, and the sorts of 'explicit' methodological pronouncements that were fashionable might certainly change over time. But the standards of correct scientific appraisal of theories are assumed to be fixed and unchanging (and therefore highly abstract and general); and it is the task of the philosopher of science to articulate these unchanging standards sharply and clearly. (One fairly recent advance has been the recognition that the philosopher of science is not likely to be able to do this by *a priori* cogitation but must instead make a detailed study of the progress of science. This does not however entail that the standards he is aiming to articulate are themselves historical, that is, subject to change.)

(ii) *These 'older' philosophers did not hold that 'axiology' governs methodology*

Laudan explicitly cites Carnap, Hempel, Reichenbach and Popper as proponents of the 'hierarchical model'. But as I understand these philosophers, they never really envisaged genuine methodological disagreements which would require arbitration at some other level; *a fortiori* they did not envisage such disagreements being resolved *via* discussion of the aims and goals of science. If two people disagree over whether or not an inference is deductively valid (and assuming this disagreement does not depend on linguistic vagueness) then one of them must be *wrong*. Similarly if two scientists disagree over which of two theories is presently better supported by the evidence, then, on the 'older' view in philosophy of science, unless this is a 'grey case', one of the scientists must be wrong. In other words, the 'older' view is *not* hierarchical in Laudan's sense: methodology governs factual disagreements alright, but there are no real, implicit methodological disagreements for 'axiology' to govern.

This seems to me fortunate since a discussion of the aims and goals of science would surely be quite unsuited to settling methodological disputes. Views about aims and goals seem altogether more ephemeral, more 'philosophical' than judgements about which scientific theory is presently best supported empirically. Indeed, and contrary to what Laudan more than once asserts, philosophers with very different ideas about the aim of theoretical science will standardly agree about which theory is currently the best available (though their *explicit* reasons for regarding that theory as best will often differ). So for example an instrumentalist and a realist will standardly agree about which theory is currently the best available. But one will claim that that best theory is our best attempted description of reality, while the other claims that the same theory is the best, most comprehensive and efficient codification of the phenomena. They will also disagree over how that best theory is to be interpreted—does it really say that there are subatomic particles or that there is a real, curved structure of spacetime; or is that all 'as if' talk? But nonetheless (syntactically) the same theories will appear in the textbooks written by the instrumentalist and by the realist.

(iii) *Kuhn (and those influenced by him) adopt a very different, because much broader, conception of methodology*

The old idea that Newton's theory was better than Aristotle's for the same general reason that Einstein's theory is better than Newton's has not, so far as I am concerned, been discredited by recent studies of science. It is true that philosophers have not yet agreed on exactly what the 'same general reason' is, and they have certainly not agreed on what exactly follows from the judgement that one theory is better than another. Nonetheless, despite the fact that their explicit methodological remarks will be very different, Duhem, Poincaré, Carnap, Reichenbach, Popper and others would all surely produce the same ranking: Einstein, Newton, Aristotle. This may not be very much but it is still, I believe, the key to scientific rationality.

Why is it so widely believed that Kuhn and Kuhn-influenced studies of science have destroyed this old idea? The answer, as I already indicated, is that Kuhn operates with a much broader conception of methodology. Of course, 'methodology' is a vague term, and many more principles than these very general abstract ones involved in ranking theories in the light of the evidence can perfectly properly be included under it without straining ordinary usage.

One broad class of principles which are certainly 'methodological' in a sense, but are not the sort of thing the 'positivists' had in mind arises as follows. An important feature of science which *did* tend to drop out of sight in earlier 'positivist' accounts is the multi-level nature of science's substantive, factual components. At any stage—especially in a well-developed science like physics—there will be a whole range of accepted theories, extending from observational and auxiliary theories (the theory of how light affects photographic emulsion, say) through specific theories (Fresnel's wave theory of diffraction, say) to more general theories (the general wave theory of light, say, that light is *some sort* of disturbance in *some sort* of mechanical medium) and on to *very* general claims of a metaphysical character (light is some sort of mechanical and deterministic phenomenon). The relationships between theories at different levels are important. For example, the levels supply at any rate a rough pecking order in case of empirical refutation. Once a general idea has proved its mettle, the standard reaction to the empirical refutation of a specific theory will be to look for a new specific theory which embodies the same general idea. Replacements for the general idea will standardly be sought only once a *series* of specific theories built around them have proved unsuccessful. For example, Fresnel's 1819 theory of light was refuted by the failure of oppositely polarised light beams to produce interference fringes in appropriate circumstances. His reaction was to hold on to the *general* wave theory of light and in fact to use the initially refuting experimental results to determine a new specific theory within that same general framework. It was only a series of failures by Maxwell and others to construct empirically adequate 'mechanistic reductions' of Maxwell's more general electromagnetic theory which forced the eventual abandonment of the idea of a mechanical medium. And even then the still more general principles, like determinism, continued to operate. Until even that principle was eventually surrendered (at any rate by most scientists).

While these more general principles—either 'scientific' like the general wave theory or 'metaphysical' like the principle of determinism—are accepted by science, they play a *dual* role: both substantive *and* heuristic. Not only are they accepted parts of scientific theory, making assertions about the world ('light is a wave motion in some medium'); they also operate as heuristic principles ('if your initial specific theory fails, look for another specific theory which still has the general features of making light a wave motion'). It by no means stretches normal usage, then, to regard these principles in their heuristic role as part of the then accepted 'methodology of science'. But if these principles *are* categorised as methodological, then of course it is no news that accepted 'methodology' has changed along with (though more slowly than) substantive science, nor that substantive, theoretical considerations have played important roles in 'methodological' change. Moreover, at any one stage in the development of science there may be 'methodological disagreements' between particular major scientists. Einstein's famous resistance to the quantum theory was over the question of whether or not the time had come to abandon determinism and the idea that all dynamical variables must *always* have well-defined values as general requirements on all physical theories. It is vital to note, however, that Einstein never for a moment questioned the present superiority of the quantum theory over all rivals—in the very abstract terms deemed methodological by positivists and others. Einstein clearly regarded the quantum theory as evidentially the best supported theory then available. It's just that he did not believe that it could be true, and hence had different views than most of his contemporaries about how that part of science should progress. Similarly, David Brewster—the most famous (and perhaps only competent) British resister of Fresnel's wave theory—readily accepted the superior empirical power of the wave theory as compared with its rivals. Nonetheless, he felt that the way forward—that is, the way to produce a new theory still better empirically supported than Fresnel's wave theory—lay outside of the general wave approach.

None of these changes in, and disagreements about, 'methodology' in this wider sense, at all challenges the views of Carnap, Hempel *et al.*, all of whom had the much narrower construal of 'methodology' in mind. Indeed these 'older' philosophers would *explain* the disentrenchment of the general wave approach in optics or the disentrenchment of the still more general principle of determinism, precisely on the grounds that scientists eventually found themselves unable, despite repeated efforts, to produce specific theories which entailed the general wave theory, or which entailed determinism, *and which maximally satisfied the unchanging, abstract formal principles of good science.*

(iv) *What counts as a methodological principle for Laudan?*

Laudan is never entirely clear about what is to count as a methodological principle. As we saw, he takes it as already established (principally by Kuhn) that there have been important changes at the methodological level in science, and plunges straight into the attempt to produce a rational model for such changes. This, I think, implicitly commits him to the 'wider view' of methodology, which includes the general descriptive/heuristic principles of the type just described. As we again saw earlier, he also seems to count as a methodological change any change in the kind of *explicit* methodological pronouncements scientists are likely to produce at any historical epoch. However his chief explicitly cited examples are of still different kinds of case.

First, as I already remarked, he claims that an important methodological change occurred in tandem with the early 19th Century revolution in optics—a shift which allegedly lifted a ban on theoretical entities and which led, for the first time, to a premium on the prediction of temporally novel facts.

This is wrong for a variety of reasons. The pre-19th Century 'ban' on theoretical entities could hardly have been part of real, implicit methodology since the universal force of gravity was certainly an accepted part of physics before the early 19th Century, and is an especially clear example of a theoretical entity. Secondly, successful prediction has surely *always* been a hallmark of a good scientific theory. Here, for example is Leibniz writing 250 years before Fresnel:

> [It is] the greatest commendation of a hypothesis (next to truth) if by its help predictions can be made even about phenomena not yet tried. (Letter to Conring, 1678)

Laudan has, I think, been misled by a certain ambiguity about 'prediction'. As I have tried to explain elsewhere (see my [1985] and [1988]) successful predictions need not be of *temporally novel* facts. The methodologically important distinction is between those empirical results which a theory 'predicts', that is, *yields naturally without any prior contrivance*, and those results for some disease, a plausible alternative is always that any observed improvement was due to placebo-effects rather than the specific action of the drug. Hence the need to perform tests 'double blind'—that is, to test the specific so long as these results 'fall out of' the theory. Here, for example, is the author of an excellent recent textbook on Newtonian mechanics:

> like any other good theory in physics, [Newton's theory of universal gravitation] had predictive value; that is, it could be applied to situations besides the ones from which it was deduced. Investigating the predictions of a theory may involve looking for hitherto unsuspected phenomena, or it may involve recognizing that an already familiar phenomenon must fit into the new framework. In either case the theory is subjected to searching tests . . . With Newton's theory of gravitation, the initial tests resided almost entirely in the analysis of known effects—but what a list! (French [1971], pp. 5–6)

This, I think, supplies the clue to the real shift Laudan has misidentified. Prior to the 19th Century the outstandingly successful scientific theory was Newton's gravitational theory. It happened that all the successful predictions in the early career of this theory were of already known effects, such as 'precession of the equinoxes'. (The successful 'novel' predictions—such as the existence of Neptune—were of course 19th Century phenomena.) In the early 19th Century another outstandingly successful theory appeared—Fresnel's wave theory of light. This had fairly immediate 'novel' predictive success—with for example the 'white spot' affair, and less ambiguously, with Hamilton's prediction of conical refraction. This put the methodological spotlight to some extent on novel predictions and occasionally confused even astute methodologists like Whewell into holding the view that there is something special about predictive novelty. But the 'something special' is, at best, psychological—and, as I have shown in my [1988], the best contemporary scientists were just as impressed by Fresnel's theory's 'prediction' of the *already known* details of straightedge diffraction as with any of its other—temporally novel—successes. The shift, then, was simply that in the 19th Century there were genuinely novel predictive successes for methodologists to become confused about.

The second example which Laudan cites as an example of a methodological change is the methodological 'discovery' of the double-blind principle for clinical trials. Clearly the double-blind principle would be treated in medical textbooks as part of the *methodology* of clinical trials—but clinical trials were not always performed double-blind. Here then is surely a clear-cut example of a methodological innovation. However, the 'positivist' could equally well tell the story of the invention of the double-blind 'methodology' while retaining his view that methodology *in his sense* remained invariant. He would surely say that it is an invariable principle of good scientific practice that, whenever possible, theories should be tested *against* plausible rival theories. The *substantive* (non-methodological) discovery of placebo-effects entailed that, for any theory which claims that a particular drug, say, has some specific efficacy which have merely been assimilated into the theory, that is, those which have been *used in the construction of the theory*. Although there is some confusion over usage, scientists indeed often talk of predictions of already known results, theory *against* the alternative placebo hypothesis. In other words, the old-fashioned position would see the innovation here as the result of plugging a new substantive discovery (that placebo effects sometimes operate) into an *invariant* methodological principle (theories should, whenever possible, be tested against plausible rivals).

In sum, then, I see nothing in Kuhn or elsewhere to challenge the view that methodology in the narrower sense has changed with the development of science. This is fortunate since, without invariant principles of good science, the whole idea of explaining the development of science as a *rational* process has surely been abandoned.

4 CONCLUSION

Assuming that changes in real, 'implicit' methodology are at issue, then Laudan's 'reticulated model', as it stands, collapses into relativism. If no principles of evaluation stay fixed, then there is no 'objective viewpoint' from which we can show that progress has occurred and we can say only that progress has occurred *relative to the standards that we happen to accept now*. However this may be dressed up, it is relativism. Without fixed standards, no amount of 'mutual adjustment . . . among all three levels of scientific commitment' can avoid it.

As he usually presents it, Laudan's 'reticulated model' presupposes no constraints on the methodological changes that *may* take place. The model *could* be saved from collapse into relativism by adding the requirement that there be an invariant core M* which is not subject to change and hence which is common to all the different methodologies M_i. Although the whole thrust of Laudan's presentation is that the 'reticulated model' is to be a definite *rival* to, indeed replacement for, the 'hierarchical' one, some remarks do suggest that he might be ready to concede that some 'core' methodological principles do indeed remain, and must remain, invariant. But then the 'reticulated model' becomes merely an *elaboration of*, rather than a rival to, 'hierarchism'. The defender of the 'older' view will, of course, readily concede that there have been methodological changes in the wider sense of 'methodology', but will insist that these very changes have themselves been adjudicated by the principles of methodology *in his narrower sense*. It is thus these principles in the core M* which are the key to the rationality of scientific progress.

If Laudan would allow that there is an invariant core M* to all his changing methodologies M_i and insist merely that his 'reticulated model' supplies a congenial framework within which to describe changes in methodology in the wider sense, then there would be no objection to it. Except that his presentation of it as a genuine third alternative totally obscures the real situation.

There is an important decision to be made: either there is an invariant core M* of methodological principles or everything is open to change. *With* such an M*, the 'reticulated model' is an elaboration, or notational variation, of the older 'hierarchical' view; *without* such an M* the model collapses into relativism. Either way, Laudan's 'third' is excluded as an independent, rival position.

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