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DISCUSSION

Fix it and be Damned: A Reply to Laudan

Larry Laudan has made a series of important studies of the historical interaction of science and philosophy of science—many of them collected in his *Science and Hypothesis* ([1981]). He holds that these studies together with the work of others in the 'historical approach' (such as Stephen Toulmin and Dudley Shapere) unambiguously show that scientific change is not restricted to the level of accepted general theories. Instead changes have also occurred both in the *methods* and *aims* of science. In his *Science and Values* ([1984]), Laudan argued that these changes in methodology and 'axiology' are inconsistent with the 'older' empiricist approach to philosophy of science, but that they do not thereby force the acceptance of 'big picture relativism'. Instead Laudan's own 'reticulated model' shows how, by piecemeal and *rational* modifications, change can spread through all levels of scientific commitment.

The obvious worry with any such claim concerns how changes can be explained as rational if even the basic principles of rationality themselves are subject to change. And indeed the main thrust of my review of *Science and Values* (this Journal, **39**, 1988, 263–75) was that Laudan's 'reticulated model' is not in fact a genuine third alternative. Instead it *either* collapses into relativism *or*, because implicitly committed to an unchanging core of methodological appraisal principles, amounts to an (interesting) elaboration of the 'older' approach. I want to explain here briefly why Larry Laudan's reply [*above* pp. 369–75] has not led to a change of mind (and also to respond to some of Laudan's criticisms of my own position).

Has methodology changed alongside changes in substantive theoretical claims? Unsurprisingly, it depends on what is meant by 'methodology'. Is it part of the 'methodology' of present medicine that clinical trials are to be performed 'double blind'? If so, then methodology has changed and changed, just as Laudan insists, in the light of substantive scientific discoveries (the placebo effect). Is it—at a more general level—part of the 'methodology' of present day physics that theories should be mathematically expressed? If so, then methodology has changed—at any rate since Aristotle. Was it part of the 'methodology' of 18th and 19th century physics that all theories be deterministic and ascribe sharp values at all times to all quantities invoked? These assumptions certainly seem to have operated in that period not only as substantive metaphysical claims within already accepted theories but also (usually implicitly) as positive heuristic principles guiding the construction of

new theories.[1] If that assumption does count as methodological then the quantum revolution—that is, a substantive scientific breakthrough—again induced important methodological changes.

I would not argue that any of these usages overstretch the elastic term 'methodology'. Moreover, I agree with Laudan that it would be 'bizarre' if changes had *not* occurred in this extended 'methodological' domain, if we had not in some sense or other learned *how to do science better* alongside doing better science. But how exactly can the new methods be judged 'better' than the old? Laudan explicitly seeks a system which will deliver this judgement and explicitly accepts that a system which fails to deliver it entails relativism. I claim that the judgement can be delivered only if some core principles (of an abstract and general kind) are considered as fixed, as *constituting* rationality. These principles will include the basic tenets of deductive logic and intuitive rules for weighing evidence (especially the principle that special weight is to be given to a theory's predictive success[2]). It is methodology in this much more restricted, core sense that I claim is fixed and must be fixed if relativism is to be avoided.

Laudan is, then, wrong that 'He and I disagree . . . about the factual claim that the methods [of science] have shifted' [*above*, p. 369]. If these methods are construed in his broad sense then no one could deny that they have shifted historically. He, on the other hand, makes it clear in his reply that it would be wrong to land him with the view that 'no methodological principles have remained invariant over the course of science (say since the 17th century)' [*above*, n. 6]. Our basic disagreement is this. Laudan [*ibid.*] 'can see no grounds for holding any particular methodological rule—and certainly none with much punch or specificity to it—to be *in principle* immune from revision as we learn more about how to conduct inquiry'. Whereas it seems to me clear that in order to make sense of the claim that we '*learn* more' about how to conduct inquiry, some core evaluative principles must be taken as fixed. (Given that these principles are intended to be of great generality, they are not going to have much *specific* 'punch'—but are nonetheless punchy enough to ground scientific rationality.[3])

1 For more details on this dual role of some statements in science see my [1985a]. That paper is in part a criticism of Shapere's attempt—in some respects mirroring Laudan's—to do without fixed 'presuppositions' in methodology. (See, *e.g.* Shapere [1984].)

2 The important sense of 'predictive success' allows that a theory may perfectly well predict an already known fact. See my [1985] and [1989].

3 These core principles must indeed be considered as very general and intuitive otherwise (as Clark Glymour urged—personal communication) the 'fixed methodology' thesis would be refuted by the case of statistical methodology. It is surely true that when statistical-probabilistic hypotheses were introduced into science they produced new and taxing specific methodological problems. The 'fixed corist' has to argue that the rules for appraising statistical hypotheses in the light of evidence are the result of applying general (and previously held) intuitions to these new and taxing cases. Without claiming to be able to argue this in detail here, I should

Can Laudan's 'reticulated model' explain scientific change as rational *without* invoking some fixed principles of logic and weighing evidence? His basic idea sounds very attractive: each part of our knowledge—substantive, methodological, and 'axiological'—is in principle revisable; but wholesale change never occurs; instead changes in one part of the 'ship of knowledge' are based on, justified by, the *temporarily* fixed other parts. Temporarily accepted theoretical knowledge may account for changes in methodology no less than temporarily accepted methodological knowledge may account for changes in theories. Thus, to take the principal historical example from Laudan's earlier work, the acceptance of the 'classical' wave theory of light (which invoked a highly 'theoretical entity'—the luniniferous ether) led to the rejection of the Newtonian inductivist methodology (which according to Laudan anathematized all theoretical entities) and to the replacement of that methodology by a more 'liberal' hypothetico-deductivism. This new methodology then went on to sanction the switch to theories which arose and were still better than the wave theory ('better' according to its own canons of course).

But however attractive the view may appear on the surface it surely fails to withstand deeper analysis. The general faults can be gleaned from the difficulties involved in Laudan's historical example. First, I believe that the claim that Newtonian 'inductivist' methodology discouraged theoretical speculation of all forms is factually false (after all it clearly sanctioned the gravitational field). But the central problem here is logical. Suppose it were true that inductivism was generally accepted in 18th and very early 19th century science and that it really banned all genuinely observation-transcendent theoretical entities. How then could the classical wave theory with its luminiferous ether (the archetypal theoretical entity) ever have been accepted? Only, it seems, by contravening the methodology then in force. Of course, *given* the (on this picture) irrational decision to accept the wave theory, then the eventual rejection of the methodological ban on theoretical entities can easily be explained—you cannot have accepted theories which clearly embody what your methodology tells you are supreme vices, and entities do not come any more 'theoretical' than the luminiferous ether. But then the relativist (again as I understand his position) does not hold that reasoning plays no role in science—that would surely be absurd—but 'only' that an essential part is played by unreason.

It might be objected that the above argument takes a very naive view of Newtonian inductivism in particular and of methodology in general. (Leplin,

point out that it is perhaps less implausible than might at first appear. How otherwise could there be an argument over which rules of evidence in statistics are *correct?* Without such general intuitions against which to test proposed statistical principles, statistics would seem to be a conventional matter, a matter of defining a new game. There has certainly been a good deal of argument over precisely the question which rules of statistical inference are correct.

personal communication.) Like all methodologies, Newtonian inductivism was a complicated affair involving subtly interconnected principles. It did not *ban* theoretical entities, but only *discouraged* theories which involved them. The methodology involved other principles and criteria: the wave theory no doubt scored highly on these. Hence the acceptance of this theory could be rationally explained; and, once accepted, the theory's clear involvement of theoretical entities could rationally bring about a reappraisal of even a principle which merely discouraged such entities rather than banning them outright.

This certainly lends the account greater historical verisimilitude. But what exactly is now being claimed? Presumably the older methodology here, no matter how subtle, *could* be spelled out. Would not the spelled-out form involve something like the principle that science should not accept theories that involve *otiose* theoretical entities—that it should accept theories involving highly theoretical entities *only if* there is some pay off for this in terms of increased (independent) empirical support? (As William Whewell pointed out, this is really the content, or at any rate the central content, of Newton's famous *vera causa* principle.) But then of course *that* principle was not at all challenged by the wave revolution (and is indeed still accepted in science). It was precisely on the grounds of the stunning empirical predictive success of the elastic solid ether theory that scientists such as Airy and Powell accepted the theory. The more accurately the episode is described, the closer we get to 'bottom-line' methodological principles which there is no historical evidence have ever shifted. These bottom-line principles sanctioned the shifts both to new theories and to new methodologies in Laudan's broad sense.

The case of another favourite example of Laudan's—the switch to 'double blind methodology' in clinical trials—is similar. No doubt, at one level of analysis, 'reticulation' occurred in this case: the placebo hypothesis emerged from science operating on the old methods and this eventually led to a revision of the methods used in the area. But how rather more precisely could this change have occurred?

Suppose the substantive claim is being entertained that—at any rate in some cases and for some people—the beliefs of those taking a drug and of those dispensing it play a causal role in symptom relief; but further suppose that the 'old methods' are still in force—that is there is not yet any methodological requirement that clinical trials be performed double blind. How could the claim now being merely entertained be validated? I can see no way of telling the story except along the following lines.

The problem is to distinguish the *extra* efect of the 'characteristic factors' of the particular drug *therapy* at issue over the effects of its incidental features (such as the beliefs engendered in the patients in its likely characteristic effect) which can be expected to be common to a range of different therapies. In order to arrive at a legitimate view of the (likely) excess effect here then we clearly need to compare the response to treatment in several experimental groups—

for example one in which the experimentees are indeed being given the drug and one in which they believe they are but are in fact being given a substance which other (presumably well-established) theories tell us have themselves no direct 'characteristic' effect on the condition concerned. Further, since any expectations the administering physician may have about the likely effect of the drug are (if operative at all) another incidental feature of the therapy, the extra effect of the characteristic features cannot be accurately gauged unless these expectations are controlled for. A fairly obvious suggestion for instituting such a control is that some method of conducting the trial be devised so that the physician does not know which substance ('active' drug or 'placebo') is being administered to any particular patient.[4]

But then, while it may issue in a new method, this whole argument is clearly underpinned (and, if reasonable, *must* be underpinned) by unquestioned assumptions about proper general, scientific methodology. Double-blind 'methodology' emerges as a particular application to a particular type of knowledge-situation of a general 'core' methodological rule. There is no more (and also no less) reason to talk of a methodological change in this case than there is in any case in which we have attributed some experimental effect to a particular factor but then newly come to suspect that some further factor may be playing a role in our experimental results and hence that we need to 'shield' our experiments against it. (For example, that we need to shield experiments from possible electromagnetic effects when we are testing gravitational hypotheses.) With an unchanging core methodology which implies that greater empirical support can legitimately be claimed for the hypothesis that a particular factor caused some effect if the experiment testing the hypothesis has been 'shielded against' other possible causal factors, then the double blind episode is easily explained as rational; without such an unchanging core, I can see no such explanation.[5]

Laudan argues in his reply, however, that it is exactly this sort of 'fixed core' position which holds 'the makings for a thick relativist stew' [*above*, p. 3]. According to Laudan, I have 'entirely misconstrued' the relativistic threat: even if it could be shown that some subset of methodological principles have

[4] See Grünbaum [1984] for an especially perceptive and careful analysis of the (as he shows, often very obscurely characterized) notions of placebo therapy, placebo control, etc. The term 'characteristic factor' is Grünbaum's: his paper should be consulted for further details.

[5] I would, then, vigorously deny Laudan's claim [*above*, p. 372] that controlled experiments 'became the norm only in the late 19th century'. What, for example, was Galileo doing in polishing his inclined planes except 'controlling' (of course, as he was aware only imperfectly) for the effects of friction? It may be that Laudan has some more specific idea of a controlled experiment in mind—perhaps the specific procedure of performing clinical trials and other physiological experiments using a 'control group'? I am not sure when systematic clinical trials got started, but surely whenever it was the idea of using controls was simply an application of old and general ideas about good scientific method.

remained fixed throughout the development of science, this would be a 'laughably feeble' response to relativism [*above*, p. 370]. Carnap, Reichenbach, Popper and co, all of whom adopted this feeble position are, according to Laudan, exactly the people who, contrary to their intentions, opened the floodgates to relativism. (Indeed it is difficult to think of any rationalist or empiricist philosopher, no matter how much of a *professed* absolutist, who fails to encourage relativism on this view of Laudan's. Certainly Descartes, Spinoza, Leibniz and Kant would all count as floodgate-openers.) To defend first principles or fixed presuppositions is, for Laudan, not to defeat relativism but to invite it. To defeat relativism we need 'to show why certain methods are better than others' and thus 'offer a justification for the current methods of science, even if they are different from the methods of science of three centuries ago' [*above*, p. 370].

Laudan thinks his reticulated model supplies such a demonstration and hence such a justification. But, to repeat the above point, how exactly? Is the 'justification' simply that our present methods turn out better *when judged from our present point of view*? But, as Mandy Rice Davis might have said, our present point of view 'would say that, wouldn't it?' The question is whether our present point of view is *right* to say that our present methods are better than the methods of science of three centuries ago. And a positive to answer that question requires some principles considered as outside the historical process. Laudan later says [p. 375] 'In my view, the history of the empirical sciences exhibits continuously increasing sophistication . . . about what sort of evidence constitutes a test of a theory . . .' But what would ground the assumption that we have *learned* more here, as opposed to simply believing that we have? What is the basis for the judgement that the empirical sciences have become increasingly *sophisticated* as opposed to degenerately baroque? Remarks like these make it seem that Laudan really belongs, without wanting to acknowledge it, to the 'fixed core' camp. To avoid joining the camp he must claim that even these judgements are grounded *only within* our present intellectual framework. But that position is classical historical relativism. Once again the 'third' position that Laudan seeks is excluded.

But whether or not he himself adopts it, is Laudan right that even the 'fixed core' position inevitably leads to relativism? If so, then it would seem that I have succeeded only in showing that, one way or the other, relativism is inevitable.

In fact, relativism as *Laudan* defines it, is inevitable. There is a potential infinite regress of justification and this means that *ultimately* the only way to avoid sceptical relativism is to dig in one's heels. How else can the sceptical relativist be prevented from forcing us down the regress by always asking for a justification of any justification he is given? Among serious people, most disagreements (at any rate of a factual nature) are, of course, resolved by finding shared standards at some deeper level: someone who starts by holding that the Biblical creation story is very likely true *may* be persuaded that in the light of deeper level standards of evidence that he shares with you, the story is in fact very unlikely to be true. But suppose instead his response to your argument is to deny your standards of evidence: he agrees that on present scientific standards his position is untenable but asks 'what's so good about science'? In the end you must stop the slide down the regress by exerting some force of your own. Somewhere along the line you just have to say that here we reach axioms and if the sceptic seriously questions them then you can help him no further and must simply (and 'dogmatically') brand him 'irrational'. Popper (in his [1945], Volume 2, pp. 230–1) was especially clear that the adoption of the rational approach cannot itself be rationally justified:

> The rationalist attitude is characterized by the importance it attaches to argument and experience. But neither logical argument nor experience can establish the rationalist attitude; for only those who are ready to consider argument or experience, and who have therefore adopted this attitude already, will be impressed by them. . . . We have to conclude from this that no rational argument will have a rational effect on a man who does not want to adopt a rational attitude . . . . But this means that whoever adopts the rationalist attitude does so because he has adopted, consciously or unconsciously, some proposal, or decision, or belief, or behaviour: an adoption which may be called irrational.[6]

Laudan cites an earlier remark by Reichenbach to much the same effect [*above*, p. 370].

Once openly acknowledged this may indeed be an uncomfortable position for a philosopher to take, but uncomfortable or not logic forces it on him. Indeed not only must the basic principles of scientific method ultimately be adopted dogmatically, so must those of *deductive* logic—as Frege, and following him Russell and Wittgenstein all clearly saw. The point was sharply emphasized ahead of Frege by Lewis Carroll. Suppose, as Carroll did in his famous dialogue between Achilles and the Tortoise, that someone accepts that p and accepts that $p \to q$ but refuses to accept q. One might try to convince him as follows: 'modus ponens in general is truth-transmitting—if p is true then if $p \to q$ is true then q must be true; here p and $p \to q$ are both true so you must infer q'. But clearly this is hardly likely to convince: if someone *really* refuses to infer q from p and $p \to q$, then it will not be surprising if he further refuses to infer that

[6] Popper was later tempted away from this (in my opinion correct) position by Bartley's development in his [1962] of 'comprehensively critical rationalism'. The basic idea of CCR was that even the claim that everything is open to criticism is itself open to criticism, and that therefore Popper's critical rationality (unlike its 'justificationist' predecessors) was rational by its own lights. But although this may sound appealing, it evaporates under scrutiny. Of course, critical rationality can be faced with all sorts of criticisms—it fails to deliver any certain truths, for example. The critical rationalist will dismiss such a criticism as unjustified (though he will avoid using the word). But what *justified* criticisms might there be? And what underpins the standards here?

he must infer q from the fact that if an argument is of a valid form and he accepts the premises then he must infer the conclusion and the fact that modus ponens is a valid form whose premises he accepts in this instance. This latter inference clearly *itself (doubly) involves modus ponens*. Like Russell, I see no way out of asserting that we know that modus ponens is (at any rate in clear cut cases) truth-preserving and either 'doubting the sincerity' of anyone who claims to disagree or being ready to brand such a person possibly sincere but definitely irrational.

Of course we are seldom forced to make this admission because we generally operate quite happily in real disputes with shared and unarticulated background assumptions. But if the sceptic really presses, then the only option is, I believe, the honest admission that *ultimately* we must stop arguing and 'dogmatically' assert certain basic principles of rationality. If Laudan is right that this honest admission entails relativism, then relativism wins. But the *serious* threat surely comes *not* from someone who simply exploits the infinite regress, but from someone who argues that if we attend carefully to the details either of logical argument or (more centrally here) to what is done in science then we shall find no single set of principles underlying the whole process but instead different principles at different times. The serious threat (or at any rate the threat that I take seriously) comes *not* from the creationist or psi-freak or whomever claiming that science rests on assertions which ultimately must be presupposed by its defenders and that therefore they and he, with his different presuppositions, stand on a par. It comes instead from one who argues that, *even in the enterprise on which his opponents bestow the honorific title 'science'*, the underlying principles have changed over time; his own principles therefore, while admittedly different from those presently accepted by science, may even become the principles accepted by the science of the near future. So why should he now give them up? The answer to such a person is precisely to show that, on the 'bottom line', only one set of principles of appraisal has ever been supposed in that enterprise that seems to all the rest of us (I hope) the paradigm of a rational enterprise.

It might seem that there is an obvious way out of having to defend our basic methodological principles dogmatically: they can instead be *argued* for along the following (Lakatosian) lines. We have certain clear-cut intuitions about the correctness of certain particular methodological judgements in science and about the incorrectness of other particular judgements in bad or pseudo-science. (Along perhaps with a whole range of cases which our intuitions leave grey.) General methodological principles can be argued for by showing that they yield the correct division between the white and black cases (*all* the white and black cases from the history of science). No doubt, as a matter of psychological fact the intuitions about particular cases are prior: it is through consideration of what our intuitions tell us about particular cases that we come to realize what general methodological principles we implicitly apply.

But *logically* speaking this does not make the situation any more comfortable for the rationalist. For suppose someone fails to share (or claims not to share) our intuitions about particular cases. Suppose someone believes (or claims to believe) that intuitively recent creationist 'science' is better than Darwinism. Unless she shares all (or at any rate the great majority) of our *other* intuitive judgements (that is, unless she implicitly shares our *general* 'first principles' and has simply made a mistake in applying them to the particular case), then there is again surely nothing to be done save branding her irrational. She is playing a different game and, we defenders of science must 'dogmatically' assert, a worse one.

Another way of arguing for the fixed core of methodological rules is by suggesting how they might have become genetically hard wired as a result of natural selection. Those who make the right inductions, those who base their actions on generalizations that have enjoyed predictive success had, and have, a selective advantage. This argument no doubt needs careful handling. But however carefully handled and however persuasive it can be made to seem, it is *clearly* circular. It is based on our belief in the correctness (or essential correctness) of Darwinian theory. But this in turn, if rational, is based on our methodological principles.

There seems to me, then, no way of arguing for our basic methodological principles that has any claim to logical priority. Assuming that they do indeed lead to the right division between black and white cases, we just assert them without argument. As Lakatos used to say (only half-jokingly) there comes a point when a rationalist must get out his machine-gun to defend rationality.

There are two further criticisms in Laudan's reply on which I should like briefly to comment. First he takes me to task (along with Lakatos) for doling out to scientists amounts of 'false (methodological) consciousness' which defy reasonable belief. The problem arises from my concession that the sorts of *explicit* methodological pronouncements that scientists are likely to make may indeed shift over time, but their basic *implicit* methodology does not shift. But, says Laudan [p. 372], the idea that 'scientists' implicit judgements about theories and evidence are virtually never wrong [while] their explicit accounts of their reasons for their theory preferences are virtually never right' is a 'monumental psychological implausibility'.

Now in fact the account I adopt needs a great deal less false consciousness than Laudan supposes. As I emphasized *above*, it fully accepts that there have been methodological shifts in the broad sense of methodology—false consciousness needs to be invoked only where scientists' pronouncements appear to go against the core of very general abstract principles of evidence. And I believe, quite contrary to Laudan, that the historical data—properly interpreted—speak to substantial continuity even with respect to *professed* methodology *at the core level.*

As I hint in my review, I do not believe, for example, that the methodological pronouncements of such 'strict Newtonians' as Newton himself and Thomas Reid are anything like as far out of line as Laudan has claimed (for example in his [1981]) with the allegedly revolutionary 'hypothetico-deductivism' of Whewell and others. (Laudan does after all face the problem that the gravitational field—surely no less a theoretical entity than the luminiferous ether—was firmly accepted during the period of alleged domination of Newtonian inductivism.) I believe that Whewell's view is more accurately represented as a new(ish) gloss on Newtonian 'inductivism' than as anything like a revolutionary new methodology.

So my (outline) answer to this criticism is that I agree with Laudan that if I had to dole out 'false consciousness' in anything like the amounts he supposes then my position would be extremely implausible. Fortunately the amounts really required by my account seem to be small indeed.

Laudan also argues that the position I advocate in opposition to his own rests on a distinction that is entirely bogus—the distinction, that is, between 'substantive' and 'formal' (or 'procedural') principles. I suggest, remember, that Laudan's cases of methodological change are better treated as ones in which substantive discoveries are 'plugged into' unchanging *formal* methodological principles in the 'old' restricted sense to produce 'new' 'methodological' views in Laudan's wider and undoubtedly substantive sense. But are there, Laudan asks, any non-substantive, purely formal methodological principles? And if, on the contrary, all methodological rules are underwritten by substantive metaphysical assumptions (even if substantive metaphysical principles of a *very* general kind) why should they be in principle unrevisable as we discover more about the world?

I admit here to having been caught out making an illegitimate simplification. It seems natural enough to distinguish principles—like 'perform clinical trials double blind'—which *clearly* rest on substantive (and relatively recently discovered) assumptions about the world, from *more* formal, more basic principles like 'test your theories against any plausible rivals that exist'. But Larry Laudan is absolutely right that even those principles we are used to thinking of as merely formal in fact rely on (in their case very general but nonetheless strictly speaking) substantial assumptions about the world. (He could even have quoted some earlier publications of mine in his support.[7])

For example, scientific procedures at the empirical level and in particular the use of empirical generalizations in technological applications surely cannot, *pace* Popper, be explained as rational unless some sort of inductive principle is adopted which enjoins the acceptance of appropriate generalizations from controlled experiments which have always turned out the same way in a

[7] See, for example, my [1983].

sufficient number of cases in the past.[8] But clearly such an inductive procedure is not going to work in all 'possible worlds'. The fact that we assume it *will* work in ours means that our theory of rational acceptance assumes something substantive about our world.

Similarly no adequate methodology for science could, I believe, fail to include a basic principle which says that non-*ad hoc* accounts should always be preferred to *ad hoc* ones (where of course both are available).[9] Suppose, for example, that a classical physicist responded to the difficulties with Mercury's motion by switching to a theory which says that every body in the universe obeys Newton's laws except for Mercury, and that Mercury moves according to some specified empirical generalization 'read off' the facts. Although he now of course has a theory which is better than his previous one in terms of empirical adequacy he surely cannot legitimately claim that the fact that he now has a theory that correctly predicts Mercury's orbit means that his theory and Einstein's theory stand on a par in respect of empirical support from these facts. But any such methodological principle which underwrote this judgement would clearly have to rest (at any rate for a scientific realist[10]) on a

8 This argument has been urged many times against Popper beginning in the 1930s with Reichenbach and Feigl; I re-argue the point in detail in my [1989a].

9 Many philosophers—including Feyerabend (see his [1975]) and some critics of the account of empirical support developed by Zahar and myself (see, *e.g.* Nickles [1985] and Howson [1985])—have misunderstood the strictures against *ad hoc* hypotheses in Lakatos' and my own work as claiming that any attempt to solve a particular problem facing a certain hypothesis is illegitimate (this of course would be absurd) or at least as claiming that it is always wrong to adopt a hypothesis which is not testable independently of the result it was introduced to explain. In fact the only claim is the comparative one that less support accrues to a non-independently testable hypothesis. Often enough, however, no non-*ad hoc* alternative is available. If so, it is of course preferrable to adopt that *ad hoc*, non-independently testable hypothesis rather than leave matters as they are. The new theoretical system will save more of the facts than its unamended predecessor. Moreover, that system may eventually be augmented so as to become independently testable (and independently confirmed) precisely in the area where the *ad hoc* hypothesis was invoked (and continues to be invoked). For example, the only explanation that Fresnel could offer within his overall wave theory for why no interference fringes were observed in the area illuminated by two closely adjacent but *incoherent* sources was that there always are objectively interference fringes but that these change from one moment to the next with such rapidity as to far outstrip the ability of our visual apparatus to record them. When Fresnel first articulated this hypothesis it was undoubtedly *ad hoc* not just in the clearly unobjectionable sense of 'addressed to a particular problem' but in the sense that it had no independent support. Nonetheless it might still be *true* and there was indeed evidence that it was true from the fact that it was the only known explanation of the phenomenon that could be given within an overall theoretical system that had elsewhere scored very striking empirical success. The hypothesis eventually became independently testable in view of developments in theories of the atomic constitution of matter and of the physiology of vision.

10 The anti-realist may appear to be better off in this respect: since simplicity is for him—allegedly—a scientific end in itself, not in need of any metaphysical underpinning. But suppose, as happens often enough, that a theory correctly accepted according to the canons of evidence shared by the realist and anti-realist makes some prediction of some hitherto unobserved general effect (a prediction *not* also made, or perhaps even contradicted, by some rival and dispreferred theory). The 'anti-realist', no less than the realist, encourages the view that—at

substantive, synthetic assumption about our universe: to put the point figuratively, who says that God did not decide to make an 'ad hoc' exception to all 'laws' of nature? Science simply assumes that he did not. (Feyerabend too rightly—and repeatedly—emphasizes this point in his [1975].)

Laudan is correct: no methodological principle is *purely* formal. But does it follow, as he suggests it does, that every such principle is open to revision in the light of further discoveries about the world? I believe that we should resist the inference from 'substantive' (and therefore 'strictly fallible') to '*seriously* corrigible'. There is evidence from the history of science of the revisability of our 'methodological principles' only if these are understood in Laudan's very broad and *highly* substantive sense. The principles from the narrower domain may be substantive, but there is no evidence that the possibility need be taken seriously that they might be revised.[11]

JOHN WORRALL
London School of Economics

the very least—it is *more* reasonable to trust this prediction, given its pedigree, than it would be were it, say, simply plucked out of the blue by some alleged 'seer'. But this clearly involves the presumption of some tie-up between the canons of acceptance and the way the universe is, or is likely to be. In fact both Duhem (with his notion of a 'natural classification') and Poincare explicitly adopt this presumption in one form or another. (No serious philosopher of science is I think properly described as an *anti*-realist. As I argue in my [1989b] both Duhem and especially Poincare—whose names top most people's lists of serious anti-realist instrumentalists—are much more accurately described as defending a *structural realism*.)

11 Laudan produces some alleged counterexamples to the 'core' principle I suggested in my review: that it is always a good idea to test our theories against plausible rivals. (In fact I do not believe that this principle is quite on the 'bottom line' but it is close enough.) The 'counterexamples' are, however, unconvincing. The principle only says that we should test against plausible rivals *if there are any*. (If—to take one of the alleged counterexamples—there were only finitely many swans in the whole history of the universe and all of them could be inspected and they were all white then there would not be any plausible rivals to 'all swans are white'.) Moreover, the principle further does *not* of course imply that testing against plausible rivals is the *only* way to argue for a theory. Laudan suggests (*above*, p. 374) that Wallis, Wren and others argued for their mechanical theories without at all involving tests against rivals. In fact, some of Wallis and Wren's arguments can, I think, properly be construed as involving tests against rivals (this need not, remember, involve new experiments and the rivals need not look plausible *once the evidence is in*) but even if they were claiming support in some other way, this does not trouble the principle.

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