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Two Cheers for Naturalised Philosophy of Science – or: Why Naturalised Philosophy of Science is Not the Cat's Whiskers

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INTRODUCTION

I am a longstanding fan of *What is This Thing Called Science?* (Chalmers 1978) But one of the things missing, aside of course from the cat's whiskers on the cover, is any really systematic discussion of the status of the theories of scientific method that it investigates. There is plenty of very sharp discussion of the correctness or incorrectness of a range of accounts of scientific method, but no explicit discussion of what sort of claim accounts of scientific method themselves are: are they a priori truths on a par with those of deductive logic or are they to be appraised empirically in the same way as 'substantive' scientific claims?

This issue has attracted much recent attention and the second sort of answer has become increasingly popular under the guise of one version or another of naturalised (or naturalistic) epistemology – the idea that the theory of knowledge, and in particular the theory of scientific knowledge, is itself somehow simply a branch of natural knowledge, like physics or chemistry; that epistemological claims, about what is and is not a good theory in science, for example, are to be appraised empirically – just like claims about, for example, the structure of spacetime or of matter.

Although there is no explicit discussion of the issue, there is the odd hint in Alan Chalmers' book that he is tempted by a view rather similar to that of Larry Laudan – a prominent naturalist. My purpose in this paper is to try to warn Alan Chalmers against any full blown endorsement of naturalised philosophy of science in his third edition. I applaud most of the motives behind the 'naturalising movement'. And I accept that some consequences of it are true and that some are valuable; and even that some are both true and valuable. However the basic theses of (fully fledged) naturalised philosophy of science simply cannot be true for they end up either in logical circles or in a particularly unpalatable form of relativism (or both). A naturalised account of method is not what Alan's third edition needs – naturalised philosophy of science is not the cat's whiskers!

2. SOME PRELIMINARY CLARIFICATION AND TWO REASONS FOR CHEERING

I heartily applaud what seems to be the chief motivation behind the recent increased popularity for naturalism: basically a mistrust of grand a priori philosophising, a distrust of the sort of philosopher, the sort of epistemologist in particular, who used to pride himself on having no interest in, and no particular detailed acquaintance with, the object-level knowledge that he philosophises about. Moreover, many of the theses associated with the many different versions of 'naturalised epistemology' and in particular with 'naturalised philosophy of science' seem to me, in some cases importantly, correct. Amongst these are the following claims:

1. The 'Copernican Principle': we humans are 'just' ordinary parts of the natural order. David Papineau, for example, characterizes the naturalist about thought and reality as taking 'the view that human beings are normal inhabitants of the natural world.. [which means he] avoid[s] theories that attribute any special status to human minds . . . [and does] not place minds outside the natural realm . . .' ([1993]) While Ron Giere asserts that 'Naturalists reject all forms of supernaturalism, holding that reality, including human life and culture, is exhausted by what exists in the causal order of nature.' (Giere forthcoming).
2. Alongside any account of what constitutes scientific knowledge, there must be – at any rate in the completed science that is the limit of rational enquiry, and in line with (1) – a purely descriptive account of how real 'cognitive agents' arrive at such knowledge; more particularly
3. Any account of scientific method had better accredit theories about humans that are consistent with (and preferably entail) the purely descriptive claim that humans have the cognitive machinery to acquire and accredit those theories themselves. (This is no more than a particular case of the requirement that any acceptable theory be consistent with the data.)

The above three theses all seem to me to fall under the category of 'obvious once you think about it, but nonetheless sometimes forgotten so worth emphasising'. They do not yet amount, though, to anything like a full-fledged naturalised view. The following two theses have also been endorsed by some self-avowed naturalists.

4. We have learned how to do science better, alongside doing better science – scientific methodology is now better informed, and better informed on account of the successes of science itself, than it used to be.
5. In particular, what we take ourselves to know already – so-called 'background knowledge' – constrains what we take to be a good new theory.

These latter two theses, which I again heartily endorse, seem to me to fall under another category: 'seemingly obvious, yet in fact likely to shed a great deal of light on the scientific enterprise when investigated in detail.'

I shall give an account later of part of the reason for categorising thesis (4) in this way; as for thesis (5), it seems to me while everyone agrees – and has agreed for decades – that evolving 'background knowledge' plays a crucial role in both the articulation and accreditation of scientific theories, and although there have been some recent hints of how powerful a detailed analysis of this process might prove to be, such a fully detailed account remains to be articulated.

So, so far one full-throated cheer for the motivation behind naturalism in philosophy of science, and one more for some of the theses often associated with naturalism. Why then, despite this and despite the fact (which I unashamedly acknowledge) that many of my best friends are naturalised philosophers of science, do I resolutely refuse to raise the third cheer? Why is naturalism in philosophy of science not the cat's whiskers?

The basic reason is very simple. Despite the complexity of the different forms of naturalism, any account that genuinely deserves to be called a naturalistic theory of science must, as again Ron Giere (forthcoming) has put it, 'reject any claims to a priori knowledge, including that of the principles of inference'. My claim, and it has already been argued any number of times in the history of philosophy, is that any account that endorses the complete rejection of the a priori either runs into logical circles (and hence incoherence) or ends up in a particularly unpalatable form of relativism. By the same token, any such account forgoes the traditional normative force associated with philosophical accounts of reasoning in science. Moreover, as I show in the final section, the theses listed above that are often associated with naturalism in fact find a much more comfortable home in the non-naturalised (or, rather, not fully naturalised view) that I endorse.

In the next section (§3) I identify what I see as the two main ways into a naturalised account of philosophy of science. In §4 I argue that both of these lead inexorably to either incoherence or relativism; while in §5 I explain why they both involve surrendering the traditional normative force of philosophy of science. Various sophisticated self-professed naturalists such as Giere, Kitcher and Laudan have all more or less clearly recognised the problems that naturalism has traditionally faced and that I here re-emphasise. They have nonetheless claimed, more or less explicitly, that these problems can be avoided – philosophy of science can after all be regarded as a fully naturalised discipline without either embracing relativism or surrendering normativity. I consider these claims in §6 and argue that they all fail: these philosophers are – unwittingly of course – guilty of promising more than can possibly be delivered. They promise the best of both worlds: freedom from a priori assumptions and yet an unambiguous underwriting of the epistemic specialness of science. The idea that both of these can be delivered simultaneously is a seductive fantasy. Finally in §7, I reaccentuate the positive by outlining in very sketchy terms the sort of minimal a priorism that I endorse and that I think is what Giere, Kitcher and Laudan ought also to be endorsing (and perhaps at root really are).

3. TWO CENTRAL THESES OF NATURALISED PHILOSOPHY OF SCIENCE

(a) *Thesis One: Methodological Rules are Corrigible Parts of the Overall System of Scientific Knowledge*

'Pre-naturalistic-enlightenment' philosophers held that methodological rules of theory-appraisal stand outside of science itself as unjudged judges of what counts as a good theory. The 'naturalisers', following Kuhn (and of course Quine), claim that those rules are in fact themselves parts of our knowledge-system, subject to change as that knowledge-system changes, having the same sort of dependence on, and corrigibility in the light of, evidence as substantive scientific theories:

> Rather than being elementary logical or methodological distinctions, which would thus be prior to the analysis of scientific knowledge, [rules of appraisal] now seem integral parts of a traditional set of substantive answers to the very questions upon which they have been deployed. That circularity does not at all invalidate them. But it does make them parts of a theory and, by doing so, subjects them to the same scrutiny regularly applied to theories in other fields. (Kuhn 1962, p. 9)

Kuhn's claim has been taken up and developed by others, notably by Larry Laudan:

> I am suggesting that we conceive [methodological] rules or maxims as resting on claims about the empirical world, claims to be assayed in precisely the same ways in which we test other empirical theories. (Laudan 1987, p. 24)

From which Laudan infers

> scientific methodology is itself an empirical discipline which cannot dispense with the very methods of inquiry whose validity it investigates. Armchair methodology is as ill-founded as armchair chemistry or physics. (Laudan 1987, p. 24)

(b) *Thesis Two: Scientific Knowledge Production is a Natural Process to be Studied Scientifically*

The second thesis that might be taken as the defining thesis of the new naturalism is as follows. Knowledge, and in particular scientific knowledge, is of course produced as a matter of fact by people – in the particular case of scientific knowledge by people we call scientists – it is therefore a natural phenomenon (what else?) and is to be studied as such with the help of whatever is the best scientific knowledge that we have about humans and their knowledge-acquiring capacities. For Ron Giere this scientific knowledge is principally from cognitive psychology, while for Philip Kitcher it consists of a broader class of theories from psychology, evolutionary and developmental biology, and even, when it comes to treating communities of knowledge-acquirers, from theoretical sociology and economics (see Giere 1988, Kitcher 1992).

4. WHY EITHER OF THESE THESES TAKEN AT FACE VALUE ENTAILS RELATIVISM

There are well known difficulties with the second thesis. When Paul Feyerabend declared that philosophers of science are essentially anthropologists with a special interest in the tribe of scientists, he was of course ready (indeed eager) to draw the relativistic consequences that seem obviously to follow. Even if a coherent set of rules could be extracted from studying those practices that scientists characterise as knowledge-gathering, other tribes – magicians, shamans, 'scientific' creationists, scientologists – who also lay claim to creating knowledge could equally well be chosen as the object of study, whereupon different rules, a different epistemology, would obviously be extracted. If we are not ready to assert a special status for rules derived from the anthropological study of scientists compared to those derived from the anthropological study of scientologists then relativism follows; if, on the contrary, we are ready to claim special status for the anthropological study of scientists, then this claim clearly cannot be based on any purely descriptive grounds – instead it depends on a prior evaluation: that is, naturalism has been surrendered.

There is a further, rather less obvious, but perhaps more insidious, form of relativism lurking within this allegedly descriptivist, naturalised 'science of science' position. Suppose that meta-level attention has been focussed, for whatever reason, on the belief-forming mechanisms of the tribe of scientists rather than the tribe of scientologists. What methods must our intrepid cognitive psychologist of science herself apply? She presumably develops cognitive psychological theories about her scientist-subjects – theories that she regards as well-supported by the evidence. Suppose now there is a rival cognitive psychologist who holds that different theories are supported by the (same) data and hence comes to quite different results in studying her chosen tribe? Must we simply solemnly record this difference of opinion, there being no way of judging one cognitive theory as better justified by the evidence than the other? If so, then, even once we have identified scientists as the proper object of study for naturalised philosophers (and who would count as such is itself clearly an evaluative matter), there need be no one correct account of their methods. Naturalism entails relativism. If, on the other hand, the naturaliser supposes that there is in general a way of correctly deciding between rival theories of scientists as cognitive agents on the basis of observation of scientists' cognitive decisions, then she is in the following, surely incoherent, position. She asserts that there are objective, non-naturalised standards for the correctness of 'naturalised', descriptive accounts of science but no such standards for object-level sciences themselves.

How about the other central naturalist thesis: the claim endorsed by Kuhn and Laudan that methodological criteria are themselves parts of the science they judge and are open to amendment along with science? Despite Kuhn's Olympian claim that the obvious 'circularity does not at all invalidate' these methodological rules, there surely do seem to be logical problems here. If each 'paradigm' comes equipped with its own criteria for what counts as a good scientific theory, and if rival paradigms involve rival criteria, then – as has often been pointed out – relativism seems straightforwardly to be entailed. Suppose paradigm 1's methodology pronounced the sort of theories it produced the best supported then available, while paradigm 2's quite different methodology pronounced as best the quite different sort of theories it produced; suppose further that paradigm 2 actually won descriptively speaking – that it was the one that as a matter of fact came to be endorsed at least by the great majority of scientists of its time. In the absence of any general supra-paradigmatic criteria that could underwrite it, this victory is merely a fact about the community of scientists. If some substantial minority of scientists continued to adhere to paradigm 1, complete with its own methodological criteria for what counts as a good scientific theory, and continued therefore to insist that the theories produced by their now unfashionable paradigm were in fact the best, then they could be condemned on no grounds other than the fact that they had been outvoted. One commentator who seems to say explicitly that this is a consequence of Kuhn's account is one Thomas Kuhn:

> When paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions... [This is] why the choice between competing paradigms regularly raises questions that cannot be resolved by the criteria of normal science. To the extent.. that two scientific schools disagree about what is a problem and what is a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms. In the partially [?] circular arguments that regularly result, each paradigm will be shown to satisfy more or less the criteria that it dictates for itself and to fall short of a few of those dictated by its opponent. (Kuhn 1962, p. 110, emphasis supplied)

This is why most of us continue to be amazed that Kuhn continued to be amazed that people accused him of propounding an irrationalist, relativist view of science.

Larry Laudan numbers among the amazed. It was precisely because Kuhn's account amounts, on Laudan's view, to 'big picture relativism' that he developed his own 'reticulated' theory of scientific change. The basic idea behind Laudan's account is that Kuhn is correct that methodologies (and even aims for science) change along with 'substantive' scientific theories, but that Kuhn is wrong that change in methodologies occurs at the same time as change in substantive theories. Each epoch in science is, according to Laudan, characterised by a triple of commitments (T,M,A) where T is a set of 'substantive' theories, M the methodological criteria then in force, and A the aims for science then adopted. Rather than scientific change occurring in all three components at once, as Kuhn seems to suggest, and which would certainly make relativism unavoidable, change standardly is more piecemeal: methodology M may, for example, remain in force while 'substantive' theory T is replaced by a rival T'. This on Laudan's account means that relativism can be avoided by providing an explanation of how and why this inter-theoretic rivalry was – at any rate eventually – 'definitively resolved': methodology M remained, for the moment, fixed, and pronounced T' superior to T. The theory T' – or some successor – may nonetheless at some later stage force a change in the methodology M, resulting in its replacement by M'.

As I argue in more detail later, Laudan never in fact gives a coherent account of how a theory, whose acceptance is initially sanctioned by some set of methodological criteria, can subsequently force a revision of those criteria. But even supposing he could, his reticulated account is, despite protestations to the contrary, no less relativistic than the Kuhnian 'big picture' view. Or rather: in so far as it gives the appearance of avoiding relativism, this is achieved only by (implicitly) espousing some fixed principles of judgment or good reason – and hence denying naturalism.

In order to see this, suppose there is a clash, or a 'tension' between theory T and methodology M. Laudan talks of a process of 'mutual adjustment' between theories and methods (and also between both of those and aims for science – but leave aims aside for the moment). Is what counts as a satisfactory adjustment between such a theory and a methodology itself subject to fixed, a priori rules? If not, then, if it happens in some particular instance that, say, T is retained and M rejected, this is simply what happens as a matter of fact. Just as in Kuhn's 'big picture', if a minority of scientists take the opposite view and hold on to M and hence reject T (or even if another minority fail to see any clash or tension between T and M!) then all one can say is that they do indeed form a minority. If, on the other hand, it is accepted that at some level, in this case meta-methodological, there are some fixed principles of good reason – good reasons for rejecting T rather than M, say, in the particular circumstances C – then the view avoids relativism, but only by virtue of failing to be naturalistic: at least some principles are assumed to lie outside the scientific game as unjudged judges.

5. WHY EITHER THESIS ALSO ENTAILS SURRENDER OF THE NORMATIVE

On the traditional view, philosophy of science aims to articulate the principles of good reason – in particular, the principles governing the evaluation of theories in the light of observational and experimental evidence. If either of the theses that characterise naturalised philosophy of science were correct, then the normative force of rules of theory-appraisal is surely lost.

The reasons are essentially the same as before. If scientific method consists just of a description of the rules that are as a matter of fact applied by those most of us regard as scientists, then the best that can be offered by way of normative advice would presumably be just that if an agent wants to appraise theories in the light of evidence in the way that scientists do, then she should apply rules R1, R2, . . . On the other hand, of course, if that same agent wishes to evaluate theories in the light of evidence in the way that 'scientific' creationists, or reflexologists or whatever do, then she should apply some quite different set of rules. No unconditional claim that some particular set of rules are the correct ones to apply when proper knowledge claims are at stake could be underwritten – except by stepping outside of a purely naturalist framework.

Suppose, turning to the first thesis, that methodological rules are themselves open to criticism and modification in the light of scientific change; suppose that some 'substantive' theory T is perceived to be at odds with some methodological criterion M; and suppose finally that this tension is as a matter of fact resolved by, say, jettisoning T and retaining M. This latter matter of fact amounts of course to no more than a sociological fact about most of those that most of us would call scientists – there will no doubt be those who think of themselves as scientists and yet remain attached to T. Were the majority right to abandon T?

Suppose, to make the point more graphic, that T is the theory that god created the universe in 4004 B.C. complete with some stuff buried in various places that looks awfully like, but is not, the bones of animals from now extinct species and complete with pretty patterns in various rocks that look awfully like, but are not, the imprints of the skeletons of animals from now extinct species and so on. M is the methodological rule that purely ad hoc reactions to experimental evidence are not to be sanctioned – or at any rate are to be strongly dispreferred when alternative non ad hoc theories are available. Most of us stick to the methodological rule that underwrites our intuitive contempt for such ad hoc dodges. But others claim that there's nothing wrong with ad hocness and therefore nothing wrong with the theory. Are we right and the latter wrong? I take it that the traditional idea that scientific rules of appraisal carry normative force implies that the answer to this question is 'yes' (or at any rate that it might be yes). Clearly this normative force is lost on the suppositions that we are making – on those suppositions, we do our epistemic thing, the creationists do theirs. There is no more to be said – unless, that is, some further, and clearly non-naturalised, assumption is made to the effect that there are correct rules governing conflicts between theories and methodological criteria.

6. CAN SOPHISTICATED NATURALISTS AVOID RELATIVISM?

(a) *Giere and Kitcher on Refusing to Play the Cartesian Game*

True naturalists, such as the sociologists who advocate the so-called strong programme, enthusiastically embrace relativism and reject normativity. Bloor and Barnes, for example, explicitly assert:

> Far from being a threat to the scientific understanding of forms of knowledge, relativism is required by it.... It is those who oppose relativism, and who grant certain forms of knowledge a privileged status, who pose the real threat to a scientific understanding of knowledge and cognition. (Bloor & Barnes 1982, pp. 21–2)

But philosophers tend to have too much respect for 'orthodox' science, and its empirical or technological success, happily to acquiesce in such a view. Both Giere and Kitcher explicitly assert that their accounts, though 'thoroughly naturalised', do not entail relativism. Giere, for example, holds (1988, p.37) that his theory shows how to 'avoid relativism without appeal to 'standards'', by instead 'focus[sing] on cognitive processes, such as those involved in representation and judgment', processes which are, he claims, 'shared by all scientists.'

But it is difficult to see how this claim can be substantiated. First of all, the idea that the processes of representation and, especially, judgment are 'shared by all scientists' will scarcely even approach truth unless a very precise selection is made of who are to count as scientists (a selection that will of course involve – if only implicitly – evaluative presuppositions). If Giere included in his study the cognitive processes of 'scientific' creationists, say, then any unanimity about judgments would clearly soon be lost.

Secondly, there is the problem of the possibility of different descriptions of the cognitive processes even of highly selected scientists. Is there one correct such description (or rather one that it is correctly accepted in view of the available evidence)? Of course, if you are selective enough about who counts as a serious scientific cognitive psychologist of science then you may well be able to approach univocality – only one description of scientists' representational and judgmental processes may be taken seriously in the light of the evidence available at a given time. But this selection too – just like the object-level one – will implicitly involve evaluative judgments, ultimately surely based on implicit judgements about which cognitive psychologists judge theories correctly on the basis of evidence – that is, ultimately implicitly based on the very norms of theory appraisal in the light of evidence, the question of whose status and validity Giere believes he can finesse.

Giere's response to these sorts of worries about alternative styles of 'cognition' by creationists, scientologists and the like is disarmingly frank:

> [Advocates of the strong programme imply] that modern science is our own version of witchcraft. Now it is one thing to reject explanations of scientific beliefs that appeal to the truth or rationality of those beliefs. It is quite another to refuse to acknowledge that our current scientific beliefs are in some important sense better than those of the past. That

> how we came to embrace our current scientific beliefs is not to be explained by appeal to their truth or to our rationality surely does not imply, for example, that we do not in fact know much more about biology and chemistry than was known in the 18th Century. An empirical theory of science need not deny such obvious facts. Its task is to explain them. (Giere 1988, p. 56)

Or again (op. cit):

> . . . the reference [by sociologists] to Azande practices is intended to illustrate the general thesis that society's image of the natural world is completely on a par with its image of the social world. Both are culturally relative, and in neither case is it possible to prove one image superior to the other. But this is just posture. If our goal is to understand the natural world in a way that makes modern technology possible, we simply must admit that contemporary scientific practice is superior to Azande witchcraft. The task for the cultural study of science is to explain why this is so, not to deny the obvious.

Now I too of course believe that it is obvious that contemporary scientific practice is superior to other ways of (allegedly) obtaining knowledge. But believing something to be obvious does not obviate the need to defend it, or at least the need to acknowledge that belief as an assumption – in this case an evaluative assumption – that one makes. Giere seems to hold that the fact that he sees something as obvious means that he has no need to defend or to acknowledge it as an epistemic liability. This would surely be indefensible even if he were right that the relativists were merely 'posturing' – but, adopting Giere's own approach, I know of no scientific evidence from cognitive psychology that suggests that strong programmers, or, at the object level, scientific creationists, scientologists and the like are insincere. In asserting that it is 'obvious' that scientific ways of proceeding are superior to alternatives, Giere is in effect simply assuming what he is intending to demonstrate, while pretending that it is no sort of assumption at all.

Suppose I wanted to argue that ethics can be straightforwardly naturalised while at the same time both avoiding relativism and retaining ethics' traditional normative force. Nothing is simpler than to follow Giere's lead. Our ethical Ron Giere would simply point out that there is no need for any normative a priori assumptions about what is ethical and what is not, all one needs to do is summon the cognitive psychologists and ask them to study judgmental processes concerning human interactions. Just as in the science case, it is important that those psychologists study the judgments about appropriate ways of interacting with one's fellow humans of the right people: avoid the Hitlers of this world and concentrate on the Nelson Mandelas (just as Giere's cognitive psychologists will avoid the Lafayette Ron Hubbards of this world and concentrate on the Albert Einsteins). Confronted with the criticism that, were one to choose instead to study the judgments the Hitlers/Genghis Khans etc, then one would arrive at a very different ethics, it could simply be pointed out that it's entirely obvious that the "saints" actions are superior to those of the rest of us; the point of ethics 'is to explain why this is so, not to deny the obvious'!

Giere does not, so far as I can tell, explicitly address the other worry: concerning how the theories of cognitive science that will be needed for the scientific replacement for old-fashioned philosophy of science are themselves accredited. Philip Kitcher does:

> If proper epistemic recommendations are crucially dependent on contingent information about the world, how could we acquire the information on which those recommendations depend? (Kitcher 1992, p. 79)

The real issue is surely how we accredit that contingent 'information' as real information, not how we as a matter of fact acquire it. But the main point here is Kitcher's response to the question he poses. Were a naturalist to try to answer by giving what Kitcher calls a 'synchronous' reconstruction of our knowledge – by which he means one that is free from logical circles of the above kind – she would eventually and inevitably 'run into unanswerable forms of scepticism'. Any such reconstruction would ultimately rely on premises – both observational and inferential; the sceptic need only question these premises to win the game (of course starting to move down the potential infinite regress by providing justifications of the erstwhile premises would itself rely on – still deeper, but still substantive premises – and hence only delay the inevitable defeat). Fortunately, with one leap Jackie is free – she should simply recognise that the attempt at justification is a mug's game:

> naturalists should . . .decline blanket invitations to play the game of synchronic reconstruction. Each of us absorbs information from our predecessors, and, through our own interactions with nature and with one another, we modify our collective picture of the world and of the proper ways to investigate it. Naturalists think of this process as leading to improvements, although there will be no way of showing that we are doing better without relying on some of our beliefs. (op. cit., p. 90)

Thus although no fan of Darwinian explanations in epistemology, Kitcher holds that one obvious objection to the use of Darwinian theory in support of epistemological claims is 'rightly dismissed'. The complaint is that one cannot invoke Darwin to underwrite any claims about the relationship between successful theory and evidence without becoming enmeshed in a circle. In accepting Darwinian theory on the basis of evidence, rather than any other view of the development of humankind, one is implicitly making the judgment that Darwinian theory is the theory best supported by the evidence; but such a judgment in turn implicitly relies on taking for granted some particular view of what it takes for a theory to be genuinely supported by the evidence; and the principles of support by evidence are precisely what one was attempting to illuminate in the first place. Kitcher – along with many other naturalists – seems to be believe that the very inevitability of such circles makes them 'non-vicious' and hence no grounds for dismissing a position:

> One complaint against the appeal to Darwin is rightly dismissed. If [critics] protest that a part of contemporary science is being taken for granted in evaluating aspects of the historical process out of which that science emerged, the appropriate naturalist reply is 'Of course,

What else?'. . . a central naturalist thesis is that some parts of our current scientific beliefs must be assumed in criticizing or endorsing others. (Kitcher 1992, p. 91)

(Presumably asking for the grounds for the 'appropriateness' of this response would uncover another 'non-vicious' circle?)

The claim, in other words, seems to be that while the naturalised view frankly rests on a logical circle, this does not matter. It does not matter because the only way to avoid such a circle would be to resume the hopeless task of confronting the Cartesian sceptic and trying to reconstruct our knowledge from foundations up and this is hopeless. So hopeless in fact that it is OK to simply change the game. Naturalistic epistemology is circular but somehow that is alright – some circles we have to live with, some circles are allegedly non-vicious (virtuous circles?).

I find the current popularity of this sort of line frankly incredible. Let's be clear: from the logical point of view, a circle is a circle – the qualifier 'vicious' is there only for rhetorical effect. Logically speaking, such naturalists are, whether they like it or not, making assumptions that cannot in fact be defended naturalistically. Logically speaking, they are making undefended assumptions (of the evidential well-foundedness of at least some scientific theories based on some principles of weighing evidence). Simply refusing to recognise this by saying that it is obvious that ultimately no such defence is possible, and hence (!) no defence is necessary seems to me the worst sort of philosophical legerdemain.

Part of the problem here is, I believe, a failure to separate logic and rhetoric. As earlier philosophers of science – notably Reichenbach and Popper – recognised, one cannot rationally defend the basic assumptions of one's rationality theory. Instead, from the logical point of view, one must regard these as asserted, but indefensible, dogmas. Here for example is how Popper put the point:

> 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 therefore adopted this attitude already, will be impressed by them. That is to say, a rationalist attitude must be first adopted if any argument or experience is to be effective, and it cannot therefore be based upon argument or experience . . . 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' ['arational' would clearly be better]. (Popper 1945, vol 2, pp. 230–1)

On the other hand, however clear it may be that this is the logical situation, if one were actually trying to persuade someone that one regarded as 'irrational' – say our favourite whipping girl, the creationist – of the error of her ways, the last thing to do would be to insist that there are certain assumptions that have to be regarded as a priori valid if one is to be properly scientific and so to insist that she is simply (and unarguably) in error in not going along with these assumptions. Instead, the sensible rhetorical tactic would be to try to get her to 'recognise' that she 'really' accepts these assumptions herself all along. Show her lots of cases from the history of science where no question of inconsistency with the 'literal' truth of Genesis arises and where – one hopes – she unquestioningly applies the same sort of evidential weighing procedures that you recommend; in particular find cases where her religious commitments are not in question and where she too dismisses blatant ad hoc manoeuvres of the 'Gosse dodge' kind. The hope would then be that, having been brought to recognise this inconsistency in her approach, she would resolve it by generalising from these other cases and hence rejecting her erstwhile acquiescence in the 'Gosse dodge'. This rhetorical ploy is, if you like, naturalistic – based on the hope of finding particular cases from science where both parties can agree, rather than operating at the first principle level – where only simple dogmatic adherence to one side or the other seems possible. The rhetorical ploy is also, when judged logically, clearly circular and therefore objectively unconvincing: there is of course no reason why – given that she was ready to accept certain ad hoc explanations within science – our creationist should not react to the above argument by making the meta-level ad hoc move and claiming that the right approach is to apply the standard scientific canons, except where the literal truth of the Bible is at issue!

In sum, in so far as they have a cogent defence against relativism, neither Giere nor Kitcher fully naturalises philosophy of science. In so far as they avoid relativism they implicitly rely on normative judgments that do not fail to play a role just because of the insistence that they need no defence on the grounds that no convincing defence could be given. It might, of course, be argued that there is little difference between, on the one hand, the claim that the epistemic specialness of science can be defended but only if we allow virtuous circular reasoning and, on the other, the claim that that epistemic specialness can ultimately only be defended dogmatically. There is, however, a no doubt small but surely significant difference: one of openness and honesty. It seems to me that the proponents of both claims are agreeing on the logical situation but proponents of the second are open about the logical situation, proponents of the first (who might be termed 'pretend naturalists') strive to conceal it. (Of course, I am not, let me hastily add, accusing Giere and Kitcher – two philosophers whom I greatly admire – nor other advocates of 'virtuous' circles of deliberate deception.)

(b) *Laudan's 'Normative Naturalism'*

The most sophisticated and detailed defence of the 'naturalistic' claim that methodological rules for science are themselves empirical and therefore modifiable in the light of new evidence as science advances is due to Larry Laudan. There are really two quite separate suggestions to be found in Laudan's work – though he does not himself clearly disentangle them.

First, there is the suggestion, developed in support of his 'reticulated model' of scientific change, that although earlier accounts took methodological rules to be fixed arbiters of theoretical claims, and in particular held that clashes between a theory and a methodological criterion are always resolved by giving up the theory, in fact scientific progress is a 'reticulated' affair: justification can go both down and up the theories/methods/aims 'hierarchy' and in particular an accepted theory may sometimes force a change in a hitherto accepted methodological criterion. The example he frequently cites here is that of the wave theory of light whose acceptance he alleges forced a change in the prevalent Newtonian inductivist account of science. The latter (allegedly) outlawed observation-transcendent notions and hence could not for long coexist with a theory which gave a central role to the invisible, intangible but all-pervading 'luminiferous aether'.

There is a superficial plausibility to Laudan's claim about this case: the acceptance of the wave theory may well have had an effect on the sort of – in Lakatosian terminology 'explicit'- methodological pronouncements that scientists were likely to make; but, as I have argued before, Laudan gives no coherent account of how a change could have occurred in the – 'implicit' – methodological rules that really govern the way scientists make theoretical judgments in the light of available evidence. Although his aim in introducing the reticulated account was to explain as reasonable changes that looked rationally inexplicable on the big picture account, Laudan fails in his aim in this case. If Newtonian inductivism (as he portrays it) were really in force at the time Fresnel developed the wave theory of light then, since that theory so obviously gave a role to the luminiferous aether, and since the aether is so obviously a theoretical notion, the theory's acceptance while that methodology was in force cannot have been reasonable. If, on the other hand, Newtonian methodology permitted theories like the wave theory, then nothing new emerged about the wave theory once it had been generally accepted that would have required any methodological changes.

A second – and quite different – sort of suggestion found in *Science and Values* but not, I think, very well articulated and defended there is as follows. Methodological rules are evidence-dependent and revisable in the light of new evidence because they are in fact elliptical conditionals: although usually expressed in the imperative form – 'prefer theories with feature F', they really unpack as conditionals such as 'if your aim is A then you will prefer theories with feature F (since it is these sorts of theory that are more likely to lead to fulfilment of A)'. Thus the rule 'Avoid ad hoc manoeuvres' is really shorthand for some conditional of the form 'If your aim is theories with cognitive virtue V, then avoid ad hoc manoeuvres'. Laudan has developed this suggestion in some detail in more recent papers and in the process has mounted an explicit defence of his claim that methodology can be naturalised and yet still retain normative force.

Once it is recognised that methodological rules are in fact implicit conditionals, then it is, on Laudan's view, easy to see why such rules are all of (i) empirical (ii) normative and (iii) revisable as science develops.

(i) The *empirical* nature of methodological rules is apparent, according to Laudan, once we recognise that they are really of the form 'if you want cognitive virtue V then prefer theories with X' since whether or not preferring theories with X is an efficient way of achieving V will in general depend on the way the world is in some respect (call this possible feature of the world W); and hence whether or not it is reasonable to prefer a theory with X will depend on whether or not it is reasonable to think that the world has feature W and this in turn will obviously depend on what those theories that have been rationally accepted in science say about the matter. Hence

> I am suggesting that we conceive rules or maxims as resting on claims about the empirical world, claims to be assayed in precisely the same ways in which we test other empirical theories... We thus have no need of a special meta-methodology of science; rather we can choose between rival methodologies in precisely the same way we choose between rival empirical theories of other sorts. (Laudan 1987, p. 24)

(ii) At the same time this view account of methodological rules allows that methodology is normative:

> one can show that a thoroughly 'scientific' and robustly 'descriptive' methodology will have normative consequences (op. cit., p. 25)

(Admittedly, this ringing claim gets qualified later as Laudan admits the obvious point that such an 'instrumental' conception of methodological rules can supply at best only local or conditional normative force: if doing X is the maximally efficient means of achieving Y and if you as a matter of fact want Y, then clearly you ought to do X.)

(iii) Finally this construal of methodological rules shows that they are subject to change and possible improvement as scientific theories improve – our view of what is an efficient means for attaining a given cognitive end may change and indeed become more accurate as our theories about world change.

There are any number of problems with Laudan's account. Of course, some conditionals of the form 'If you want X, do Y' are sensible only in view of contingent facts about the world: 'if you want your toy balloon to float, fill it with helium' is only good advice to your child, because of a feature of the world that is clearly contingent – that helium is lighter than atmospheric air. But does this hold of any plausible conditionalised version of a methodological rule?

One alleged example that Laudan gives is that of the rule – which he attributes to Popper – of avoiding ad hoc hypotheses. He suggests that this rule is really a conditional: 'if you want to have risky theories, then avoid ad hoc hypotheses'. But the connection between the antecedent and consequent of this conditional is not of course empirical (or even partly empirical) it is purely conceptual – nothing in the way the world is could make an ad hoc theory risky, 'ad hoc' means tailored to already known evidence (and if the evidence is already known and the theory tailored to it, it follows of course that the theory is at no risk from that evidence).

Other plausible 'cognitive virtues' that spring to mind are simplicity, empirical adequacy and truth (though Laudan would dismiss the latter two as reasonable aims on the grounds that they are utopian). But what values of the variable X in 'if you want a theory that is simple/true/empirically adequate prefer theories with X' might make such a conditional plausible and at the same time dependent for its acceptability on which theories we accept about some substantive feature of the world?

I shall try to articulate the important point that I think underlies Laudan's view in the final section of this paper. The main critical point I want to argue here is that even if it were true that methodological rules are elliptical conditionals and even if it were true that the validity of these conditionals were dependent on empirical considerations, Laudan would still have failed fully to naturalise methodology and failed to retain the traditional normative force of methodological criteria.

Even if both these highly debatable assumptions are conceded, Laudan's account loses the traditional normative force of methodological criteria unless supplemented by some account of what acceptable aims for science, or what 'cognitive virtues' in theories really are virtues. Very strange methodological rules would, after all, be sanctioned on Laudan's account if there were no restriction on aims. Try: 'Prefer theories that are inconsistent with a range of well-established low-level empirical results'. This might seem to be a very strange methodological rule. But it can easily be construed as a perfectly sensible elliptical conditional, if a permissible 'cognitive virtue' at which to aim is almost certain falsity.

Laudan in fact acknowledges that his account is incomplete without a proper 'axiology'. What he fails to emphasise is that this is bound to involve old style traditional epistemological considerations. Even his own – in my opinion – misguided hints on this subject confirm this. He suggests, for example, that one restriction on permissible aims is that they should not be utopian; but, even assuming this to be correct (it is not), how could naturalistic empirical considerations underwrite this restriction? I have no doubt that methodological rules may sensibly be regarded as elliptical conditionals, but only if the aim in the antecedent of the conditional is in fact always the 'utopian' (or at any rate non-effective) notion of truth or complete empirical adequacy.

Once again: Without restrictions on appropriate aims for science, Laudan's account surrenders proper normative force; by acknowledging such restrictions it surrenders naturalism.

Finally if it were true that methodological rules are conditionals and if it were true that empirical knowledge is needed to sanction such conditionals and supposing, as we have been throughout, that what counts as knowledge is characterised by its satisfaction of the appropriate methodological rules, then a fairly obvious regress problem arises. Presently accepted theory T sanctions the conditional methodological rule 'if you want virtue V prefer theory with characteristic C'; of the available rival theories in some further area S is the only one with characteristic C and so S is accepted. But how did T get accepted? Presumably on the basis of some methodological rule (or rules) with, on Laudan's account, the same conditional form and therefore requiring backing from some still further accepted theory (or more likely set of theories) U; but how did U become accepted. . .?

Again Laudan seems to confront this issue:

> But, my philosophical critic may be quick to point out that . . . [my account] thus far ignores the fact that we could 'test' a methodological rule only by taking for granted the prior establishment of some other methodological rule, which will tell us how to test the former. [Presumably by warranting the required information that doing X is likeliest to promote Y]. And that latter rule, in its turn, will presumably require for its justification some previously established methodological rule, etc. We seem to confronted by either a vicious circularity or an infinite regress, neither of which looks promising therapy for our meta-methodological anxieties. (Laudan 1987, p. 25)

Laudan's reply is revealing:

> The quick answer . . . is that we can avoid the regress provided that we can find some warranting or evidencing principle which all the disputing theories of methodology share in common . . . I believe that we have such a criterion of choice in our normal inductive convictions about the appraisal of policies and strategies. In brief, and for these purposes, those convictions can be formulated in the following rule:
> (R1) If actions of a particular sort, m, have consistently promoted certain cognitive ends, e, in the past, and rival actions, n, have failed to do so, then assume that future actions following the rule 'if your aim is e, you ought to do m' are more likely to promote those ends than actions based on the rule 'if your aim is e, you ought to do n.' (ibid.)

Laudan's formulation of the basic claim behind 'our normal inductive convictions about the appraisal of policies and strategies' is – to say the least – arguable. But, aside from details, his move here surely gives the game away. If one is allowed to take for granted our 'normal inductive convictions' then there is little problem in justifying them. Surely the whole point of philosophy of science has been to try to articulate and defend these 'inductive convictions'. Since Laudan seems to endorse those convictions, he is of course taking a non-descriptivist stance: someone lacking them as a matter of fact would not, as a matter of evaluation, act in a genuinely scientific way.

Laudan's own attempted justification of the rule R1 is first that it is 'arguably assumed universally' and, secondly, that 'quite independently of philosophical consensus it appears to be a sound rule of learning form experience. Indeed if (R1) is not sound, no general rule is.' Hence it provides what he calls a 'quasi-Archimedean standpoint'. Well, Popper for one would as a matter of fact deny that R1 is a rule applied in science. It might very well be argued that whether Popper likes it or not, he must in fact (that is, in normative fact!) assume R1 or something more sophisticated but similarly inductive. But this argument would of course have to proceed in a traditional philosophical, non-naturalised way. Indeed Laudan himself offers such an entirely traditional argument – essentially Reichenbach's attempted vindication of the infamous 'straight rule'. But this is where we came in – this is where all the action was before naturalised philosophy of science came along. Underneath the so-called naturalised view, lurk normative assumptions of the traditional philosophical type and therefore philosophical problems of the traditional type.

I have argued, then, that naturalised philosophy of science – whether it is taken as chiefly characterised by the thesis that knowledge production is a natural phenomenon to be studied like any other or by the thesis that methodological rules have no special, supra-scientific status – promises more than it can deliver. It promises to deliver us from traditional a priori assumptions and hence from traditional problems, but, on critical examination, does no such thing.

7. 'MINIMAL A PRIORISM': THREE NOT-FULLY-NATURALISED THESES

I have argued that the striking headlines of the positions adopted by naturalisers like Giere, Kitcher and Laudan are misleading and often enough belied by what can be found in the small print of their accounts. One response would be that the 'small print' is to be taken just as seriously as the headlines. These philosophers do talk of adopting 'thoroughly naturalised' views; and Giere states explicitly that such a view 'reject[s] any claims to a priori knowledge', while Kitcher explicitly endorses the thesis that 'No epistemological principle is knowable a priori' as an essential part of naturalism. Perhaps, however, these were unfortunate exaggerations and the position they really adopt is rather more sophisticated. Suppose then that I have been attacking strawmen. The issue would then be – given that we all now agree that one cannot fully naturalise philosophy of science – that of identifying the strongest defensible theses that can be extracted from 'naturalised' accounts. There are, I believe, three such theses. They together illustrate a view that might be called 'minimal a priorism'. I shall end by briefly, and inevitably rather sketchily, outlining these three theses.

(a) *Philosophers of Science Should 'Start' from Particular Cases*

The basic idea here, one argued by Lakatos for example, is that we have a firmer grip on intuitions about particular evidential judgments than we do on general principles of the logic of evidence; hence, as a heuristic matter, it is a good idea to start with particular cases that seem especially clear and try to arrive through them at the general principles. Clearly the relevant evidence at the time gave great support to Newton's theory, to Maxwell's theory and so on; while claims about support for creationism, pyramidology and the rest are clearly spurious. We can even talk – as

Lakatos did – of 'testing' our general principles of evidential reasoning against these cases: an adequate set of general principles must underwrite the clear-cut positive particular cases (Newton, etc) must exclude the clear-cut negative particular cases ('scientific' creationism etc) and will in the process resolve grey cases.

Notice that, some of its later press notwithstanding, this is not a fully naturalised view. For one thing, the individual judgments are indeed value judgments (the 'basic value judgments of the scientific elite', Lakatos calls them). For another, the general principles that are somehow 'extracted' from these initial, particular judgments must surely have some inherent plausibility and be given some plausible defence: otherwise one could justly be accused of simply prejudging the game – by making Newton and the rest scientific, and creationists and the rest unscientific by definition.

There seem to me to be in fact no differences in this regard between the methodological rules we are discussing – what we might call (in the loosest sense) the rules of inductive logic – and the rules of deductive logic. The latter were surely developed in this same way. Certainly, had Aristotle come to me for advice on how to go about discovering the general principles of deductive validity, then I would have advised him against lying on his couch and trying to excogitate those principles 'a priori', but instead to think of particular arguments that are taken as valid by himself and others he thinks of intuitively as really smart, and to think of other particular arguments that seem similarly invalid; he should then go on to try to think what the intuitively valid arguments have in common that is not shared by the intuitively invalid ones. But this is a purely heuristic suggestion – although I am sure that this is the most sensible method for arriving at the general principles of deductive logic, it does not of course follow that the justification for those general principles once articulated is similarly 'a posteriori'. Philip Kitcher's naturalism includes the claim, as we saw, that 'No epistemological principle is knowable a priori'. I am ready to concede the possibility that an even more general claim might be true: namely that no principle is knowable a priori. But only if this is interpreted as the descriptive claim that experience is psychologically necessary for coming to appreciate the truth of what are logically speaking a priori principles.

(b) *Methodological Criteria in Science do Change and Indeed Improve*

The basic idea here is best illustrated and clarified using one of Laudan's own favourite examples. We now definitely have a better methodology for testing the specific efficacy of drug therapies than existed earlier in the history of scientific medicine. It is nowadays generally accepted that such tests provide more convincing evidence (either for or against the efficacy of the therapy) if performed 'double blind'. This requires that the set of experimental subjects be divided into two: one sub-group who receive the therapy under test and the other who receive either 'placebo' or conventional treatment; and that both the subjects themselves and those administering the therapies must be unaware of the sub-group to which they belong (that is, both a patient receiving a therapy and the doctor administering it must not know whether the patient is receiving the therapy under test or placebo (or conventional) therapy). The introduction of double blind methodology was an innovation – trials of therapies have not always been performed in this way.

But am I not contradicting my earlier criticism of Laudan by this endorsement of the view that we here learned to do science better as a result of doing better science? We need to make a distinction between the general formal rules of methodology, which I claim are fixed and non-'naturalised', and specific applications of these rules to particular cases which not unnaturally depend on the details of the particular case and therefore on our changing knowledge of those details. Less this distinction seem ad hoc, let me motivate it by looking at what I claim is a formally entirely analogous situation in ethics.

We are, I believe, in the midst of an ethical revolution. Twenty years ago smoking cigarettes in public places raised not an eyebrow (though it did raise many a cough); I predict that soon (though not soon enough) this will be generally regarded as morally unacceptable (of course not the greatest crime against morality one could envisage but definitely 'not done'). Clearly this ethical revolution will have had everything to do with empirical evidence – in particular about the effects of so-called secondary smoking: the adverse effects on health of inhaling other people's cigarette smoke. Does this mean that all those who insist that ethics cannot be naturalised, that 'is' never entails 'ought', are wrong? Does it show that ethical principles are open to empirical revision on a par with other more obviously descriptive empirical claims?

Well, the anti-naturalisers in ethics may be wrong – I am certainly not arguing for their view here, but I do argue that they are not wrong simply on this account. Surely what will have happened, if my prediction is correct, in this case is that new empirical knowledge – that inhaling other people's cigarette smoke can have adverse effects on one's health – was plugged into a more general ethical principle which remains fixed and unchallenged by any empirical evidence (at least in this episode). This general principle says something like it is wrong to risk adversely affecting other people's health simply for one's own passing gratification, especially when that gratification can be achieved in private without running that risk. What has (or will have) changed is not the basic ethics, but rather our knowledge of which particular gratificatory pursuits fall in this general category of risks to other people's health: we didn't use to know that cigarette smoking fell in this category, now we do.

This shows that particular ethical judgments are dependent on empirical knowledge and the growth thereof; but it seems to me that if we are to argue that this was an ethical advance, we must assume that there are fixed underlying principles that are not affected by empirical evidence but into which new empirical information is plugged so that new specific ethical judgments can be inferred.

The case of double blind methodology is formally entirely analogous. The real underlying, general and formal principles that govern all evidential judgments remained the same, but by plugging new empirical information into those judgements we infer new more specific, more 'substantive' methodological principles. The basic principle involved here asserts that it is illegitimate to infer that some change in one variable (symptoms, say) is caused by a change in another variable (administration of new therapy) unless other variables that might plausibly also play a role have been 'controlled for'. (This amounts in effect to a version of the principle that theories should always be tested against any plausible alternatives.) The change here then resulted from a series of purely empirical discoveries – that patients' expectations that a therapy might make them better can play a role in actually making them better (the 'placebo effect'), that such expectations can be generated by their attending physician's expectations, and that – especially in cases where outcome measurements are somewhat judgment-dependent – investigators tend subconsciously to overestimate the effects of a therapy which they are subjectively convinced is efficacious and tend subconsciously to select patients for the 'active' arm of the trial whom they think are specially likely to benefit or who are specially likely to prove strong enough to withstand any negative side-effects. Plugging these purely empirical discoveries into the fixed general principle yields the double-blind recommendation.

As in the ethics case, if the implementation of this recommendation is to be explained as the advance that it surely was then the story must be told, I would argue, in the way just described. The advance consists in the fact that we always need to control for possibly relevant variables and we discovered new variables that might be relevant.

This all explains why methodology in the form of quite specific recommendations – like test new drug therapies in double blind trials – can be informed by and change in the light of empirical advance; but only because general methodological principles – like always test theories against plausible rivals – stand outside the fray as fixed arbiters of evidential judgments. 'Big methodology' (consisting of these more specific, contentful recommendations and criteria) is dependent on empirical knowledge; but we can talk about advances in it only because 'little methodology' (consisting of the general, formal principles) is not and cannot be 'naturalised'.

(c) *Meta-Level Induction on the History of Science*

There is a further sense in which it seems plausible to claim that we learn how to do better science as we do better science. But, although often associated with naturalism, this claim again only makes real sense if we assume that certain principles at least are constitutive of good reason in a way that is fixed and outside the empirical fray.

Lakatos liked to point out that there is no a priori reason why God should not have decided to make the world Newtonian, except for seventeen exceptions; that we have, in other words, no a priori reason for dispreferring non-unified, ad hoc theories. Why then is this dispreference an important methodological rule that has governed much of the development of modern theoretical science?

The first part of the explanation, I suggest, is that there is a more basic methodological rule – one that bids us prefer theories that have predictive empirical success, theories, that, having been developed to deal with phenomena in one area, have turned out to have testable implications in another area that turned out on actually performing the relevant tests to be correct. The second part of the explanation is that there is evidence from the history of science that it is by looking for a new 'unified' theory which did not permit ad hoc exceptions that scientists have managed on the whole to produce such theories. If we didn't have a history of mature, successful science we should have no basis for thinking this an appropriate rule. Putting the same point more metaphysically that rule would only be successful in a universe that has a certain contingent structure and our evidence that our universe does indeed have this structure comes in the form of reports about the sorts of theories that have been successful in the past.

This point needs further clarification than it has so far received, but it clearly endorses some of what naturalisers like Laudan say. Notice however that, once again, although this endorsement might seem like a concession to the naturalists, the whole story of such meta-level inductions only makes sense against a background in which certain formal methodological principles are taken for granted – as, in other words, not themselves open to empirical question. One of these is some sort of inductive principle (as the Laudan small print allows, as we saw); the other is the preference for predictive theories.

As I hope this last section makes abundantly clear, I really do raise two cheers for naturalised philosophy of science – naturalisers are pointing to important aspects of the scientific process which, if not new, have often attracted less attention than they deserve.

I reserve the third cheer because these important aspects of the scientific process fail to amount to a naturalised view in any full-fledged sense – instead, they can be made sense of, in a way that in particular avoids a particularly dire and untenable form of relativism, only against the background of non-naturalised general principles of the logic of evidence. Those sharing my abhorrence of grand a priori 'first philosophy' and properly realising that good philosophy of science is only done by paying attention to the details of science itself should resist the temptations of fully-fledged naturalism. The heaven it appears to offer – of a defence of science and of straight thinking generally without recourse to any a priori assumptions – is a fantasy.

The logically clear-sighted and scientifically informed view is a minimal apriorism: the programme should be that of identifying and defending the minimal core of a priori assumptions that need to be made to defend scientific rationality and of showing how more specific methodological principles indeed follow from scientific advances plus this core of assumptions. This, I believe, is, despite the fully fledged naturalist rhetoric, really the programme on which Larry Laudan, Ron Giere, Philip Kitcher and others are embarked. I wonder whether Alan Chalmers feels that he too belongs in this same programmatic boat.

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