REVIEW

AGAINST TOO MUCH METHOD

Review of P. K. Feyerabend *Against Method*, London: New Left Books, 1975, £4.50.

Paul Feyerabend's book promotes 'epistemological anarchism'. This, it transpires, is a sort of scepticism for the intellectually energetic. The sceptic claims that no proposition about the world has epistemological superiority over any other proposition; and he concludes that the rational man should suspend judgement (on everything). The epistemological anarchist apparently does no more than rephrase the sceptic's claim to read: 'Every proposition is epistemologically on a par with any other'. But he draws a very different conclusion from this claim. He knows that no theory, no theoretical system, no approach to knowledge is better than any other, but he needn't tell anyone this. Instead he may defend any theory, any approach to knowledge that he likes to suit whatever purposes (of a non-epistemological kind) that he may have.

> [T]he epistemological anarchist has (writes Feyerabend) no compunction to defend the most trite or the most outrageous statement . . . He will on occasions be the most vociferous defender of the *status quo*, or of his opponents . . . His aims remain stable, or change as a result of argument, or of boredom, or of a conversion experience, or to impress a mistress, and so on. Given some aim . . . he may use reason, emotion, ridicule, an 'attitude of serious concern' and whatever other means have been invented by humans to get the better of their fellow men. His favourite pastime is to confuse rationalists by inventing compelling reasons for unreasonable doctrines. There is no view, however 'absurd' or 'immoral' that he refuses to consider or to act upon . . . (p. 189). [Although he] opposes positively and absolutely [such universal standards as truth and reason,] he does not deny that it is often good policy to act as if such standards existed, and as if he believed in them (*ibid.*).

It would be something of a miracle if someone presenting such a position managed to avoid inconsistencies, especially if he presents the position over more than 300 pages. It must be extremely difficult constantly to remind oneself that one's basic position gives one no right to assert any thesis positively, no right to assert that one position is better than another, nor even any right to claim any rational cogency for one's arguments. It would therefore be rather easy to score debating points by exhibiting inconsistencies in Feyerabend's exposition.

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Indeed in the few brief passages already quoted there are at least two such inconsistencies. Surely the epistemological anarchist cannot consistently oppose anything 'positively and absolutely'. And yet, as we just saw, Feyerabend's prototype does just that regarding truth and reason. Also we have just seen Feyerabend talking of 'trite' and 'outrageous' statements, and there are many places where he clearly assumes that genuine scientific progress was made in certain cases. But this seems to imply distinctions between trite and contentful, between outrageous and well established, and between scientific progress and mere scientific change. The epistemological anarchist ought to reject all such distinctions. Again there are places where Feyerabend clearly asserts the superiority of epistemological anarchism over other methodologies. This is either inconsistent or entails the rather strange view that while there are no objective standards for 'object level' theories (theories of physics, chemistry, social science, *etc.*) there are such standards for 'meta-level' epistemological theories.

Pointing to these kinds of inconsistency seems to me of little interest, however, for although the Feyerabendian letter may be inconsistent, the spirit can usually be preserved quite easily by local patching.

Just as the Pyrrhonian sceptic avoids inconsistency by making his scepticism self-referential so Feyerabend can extend his anarchistic attitude to his own epistemological position. He would then say that epistemological anarchism is at least as good as any other epistemology and there is no reason why he should not make propaganda for it. All the passages in which he seems to claim superiority for epistemological anarchism are to be interpreted as propaganda exercises.

Similarly where Feyerabend seems to be admitting that there is genuine progress in science he can easily claim to be playing the rationalist's game. He can say that he is showing that *even if* we accept the rationalist's claim that the step from, say, Aristotle to Galileo was genuinely progressive, then we can still show that it was not achieved by 'rational' means.

Indeed *nearly all the time* this is precisely what Feyerabend *does* say and so to harp on minor inconsistencies would be particularly unfair. One example:

> note that progress is here defined as a rationalistic lover of science would define it . . . Of course there is no need to accept this definition . . . We use it only to show that an idea of reason accepted by the majority of rationalists . . . may prevent progress as defined by the very same majority (p. 156).

There are also passages which indicate that Feyerabend is aware of the possibility of the first type of inconsistency too and would deal with it as I indicated: interpreting what seem to be rational arguments for epistemological anarchism as propagandist moves.

Feyerabend's position is, then, consistent (or at least there is a consistent version of it which has all its main features). It is also, for me at least, extremely unattractive. What reasons might someone who starts as a 'rationalist' have for taking it to his breast nevertheless? Feyerabend, again playing the rationalist's game, tries to produce several such reasons.

Feyerabend's central arguments can, I think, be reduced to three. The first, and only positive argument is roughly that epistemological anarchism would be good for people, especially those whose brains have shown an unfortunate tendency to 'ossify' and those who are intellectually 'constipated', and it would be good for societies (particularly those in which the scientific establishment has achieved too much power).

I find it difficult to take this argument seriously. First, many of the social consequences Feyerabend envisages are likely to be regarded by most people as constituting arguments against his position rather than for it. Amongst these, at least for this 'boneheaded', 'constipated', 'unthinking slave of the establishment' are the ideas that there should be increased state intervention in science and that parents should have the right, if they wish, to insist that their children be taught voodoo instead of science in schools.

But, more importantly, these arguments are, from the logical point of view, entirely bogus. These alleged social consequences are quite independent logically of Feyerabend's epistemological position. It is quite possible that everyone became epistemological anarchists without this having any effect on society whatsoever. Everyone may start to 'Do their own thing', but this may turn out to be precisely to continue what they had been doing all along. Indeed, the proponent of epistemological anarchism has no right to advocate *any* social change (although the epistemological anarchist himself may propagandise for any social policy that takes his fancy). He knows that he does not (and cannot) really know what is good for people but he may, of course, pretend that he does.

Feyerabend's second argument is more serious. The conclusion of the argument is this:

> there are situations when ... [even] our most liberal [rational methodological] rules would have eliminated an idea ... which we regard today as essential for science ...

and such situations occur quite frequently . . . The ideas survived and they can *now* be said to be in agreement with reason. They survived because prejudice, passion, conceit, errors, sheer pigheadedness . . . *opposed* the dictates of reason *and because these irrational elements were permitted to have their way.* To express it differently: *Copernicanism and other 'rational' views exist today only because reason was overruled at some time in their past* (p. 155).

This is in many ways the central argument of the book: most of the first fourteen chapters are taken up with developing and illustrating it.

Feyerabend's method of argument is this. He takes various developments in science which his 'rationalist' opponents would intuitively regard as progressive. He takes various of the 'rationalist' methodological rules that have been proposed. He claims to show that adherence to any of these rules would have prevented the progressive steps, and thus that 'progress' was only *in fact* brought about 'counter-inductively' by breaking the rules. One particular historical example, the move from Aristotle to Galileo *via* Copernicus is considered at great (and often fascinating) length. Amongst the methodological rules which Feyerabend claims to show were 'rationally' broken are the rules against proposing theories which are inconsistent with already well-confirmed theories (but who would now defend that rule?); the rule against theories which are inconsistent with well-established empirical data; and the rule against the sort of *ad hoc* immunising moves which (in response to experimental difficulties) introduce theories of lower content than their predecessors.

To get a better idea of Feyerabend's claims let us consider his favourite example. Aristotelians apparently felt that the theory of the diurnal rotation of the earth (a part of Copernicus's theory) was refuted by the fact that a rock allowed to fall from the top of a tower does not hit the earth many hundreds of yards to the west of the base of the tower (as it was alleged it ought according to Copernicus's theory) but rather lands at the base of the tower. This certainly seems to have been a widely used argument against Copernicus. Let us for the moment go along with Feyerabend's assumption that these Aristotelians were correct and that this observation is indeed inconsistent with Copernicus's theory. Feyerabend draws the conclusion that, by adopting Copernicus's theory despite this refutation, Galileo broke the rule against theories which are inconsistent with well-established empirical data. And, continues Feyerabend, a good job too! For Galileo was eventually able to show that this observation involved interpretative elements (it involved a 'natural interpretation') and once he had reinterpreted the observations (by smuggling in a new 'natural interpretation') the refutation simply disappeared.

Let us assume that Feyerabend's historical account is correct. What precise rule can Galileo be said to have broken? There are, it seems to me, two possibilities: Rule 1 and Rule 2. Rule 1 reads: 'Eliminate from your mind any theory which is inconsistent with some well-established empirical result, do not even try to develop that theory into something better.' No remotely acceptable methodology could incorporate such a rule. Even if we were to accept that an empirical refutation demonstrates the inferiority of a theory, then, since 'working on' a theory means trying to develop a different theory 'on the basis of' the old, to infer that any such new theory must also be unacceptable would be to commit a particularly obvious genetic fallacy. The fact (if it is one) that Galileo broke Rule 1 embarrasses no reasonable methodology. (I don't deny though that it is tempting to claim Rule 1 type implications for appraisal rules. Feyerabend catches several methodologists (notably Lakatos) falling prey to this temptation.)

The second rule that Galileo might be said to have broken, Rule 2, reads: 'Do not accept as a candidate for the truth a theory which is inconsistent with some well-established empirical statement'.

As I shall show later Galileo can be said to have broken Rule 2 only if we allow quite high-level statements to count as empirical. Feyerabend's account would certainly provide a difficulty for any methodology for which all such statements are incorrigible. But it provides no sort of difficulty for any methodology for which they aren't.

Compared with these two interpretations, the interpretation of the 'rule' against empirical inconsistencies which I would advocate is much more modest. It 'instructs' the scientist as follows:

> You may find that the theory you adopt is at various times inconsistent with accepted factual statements. Something has to be done about this, for one thing you may be sure of is that two inconsistent statements cannot both be true. If your theory is to be accepted in the long run, you will have to show that the conflicting factual statements are, despite appearances, false. (But a word of encouragement: *either* the alleged factual statement will be inconsistent not with the theory alone but only with it together with lots of auxiliary theories, *or* it will be

> found to be highly theory-impregnated; and so in either case you will have several points of attack.)

As for the rule against *ad hoc* stratagems, again if this is interpreted as instructing the scientist never to use such stratagems, it is little wonder it is historically refuted. The more modest interpretation I would recommend here is this:

> You may discover at various times that the only explanation you can find within your theoretical system for some recalcitrant fact is an *ad hoc* one. Do not rest content with this but try to find a non *ad hoc* explanation, for only non *ad hoc* explanations can increase our knowledge.

Feyerabend's historical examples do not hit these rules if interpreted in these more modest ways. In order to hit *these* rules, Feyerabend would have to find a Galileo actually claiming that there is nothing wrong either with his theory or with the factual statements inconsistent with it, or a Galileo rejoicing in an *ad hoc* manoeuvre. Feyerabend, of course, finds neither. Indeed his examples far from embarrassing these rules actually support them.

As Feyerabend demonstrates, Galileo set out to show that when the 'facts' are properly interpreted the inconsistency with his theory disappears. Galileo indulged in *ad hoc* manoeuvres, according to Feyerabend, only to "give his theories a breathing space", and with the intention of developing his theory (and related auxiliary and observational sciences) so that the new theoretical system would deal correctly with many more, and not fewer, phenomena. It seems clear to me that to adopt short term measures which go against certain standards (and with which one is anyway dissatisfied) with the longer term intention of developing a theory which does satisfy them is to uphold the standards, not to demonstrate their uselessness.

Feyerabend's response to this argument would no doubt be that to advocate only such modest rules is really to adopt anarchism. Indeed he admits (pp. 181–186) that if one gives up altogether the hope of providing methodological rules (in his sense) and sticks simply to trying to produce a system for appraising theories then one avoids the force of his historical examples. One does so, however, at the expense of adopting epistemological anarchism.

All methodologies provide systems of theory-appraisal. Most methodologists hoped that these appraisals would have *some* practical consequences: for technology (which theories to rely on?), for science (which theories to work on?) or for funding agencies (which theories to fund research on?). This hope has, in my opinion, turned out to be a vain one. None of these kinds of practical consequences follow from appraisals of the present merits of scientific theories unless there is added some extra premise, some 'inductive principle' connecting past success with future performance. And rational arguments in favour of such 'inductive principles' seem difficult to come by. Many people seem to agree with Feyerabend, however, that if methodological appraisals have no such consequences they amount to little more than handwaving. This seems to me quite wrong. First, some (admittedly rather modest) rules which are immune to Feyerabend's arguments can be formulated and defended (as we have just seen); secondly, a system of theory appraisal is much more consequential than Feyerabend suggests. For instance, although such a system may not tell the scientist how to go about constructing successful theories, it will tell him what general features a new theory must have in order to be successful. Again, both parties in a debate may claim that the scientific weight of evidence is presently on its side. A system of theory appraisal can tell us which side's claim is correct. This does not imply that the other side's cause is hopeless, but this in turn does not imply that the appraisal is inconsequential. (Moreover, although the straightforward inference from 'theory *A* is better than *B*' to 'work only *A*' is no doubt (as I argued earlier) absurd, this does not mean that methodologies whose primary aim is to provide systems of theory-appraisal must remain entirely silent on heuristics. If for example, an advocate of the methodology of scientific research programmes finds that, according to his appraisal rules, programme *A* has both a great deal more empirical support and a much stronger heuristic than programme *B* then he might sensibly give the following 'advice' to someone considering working on *B*: 'Those heuristic ideas provided by programme *B* for developing new better confirmed theories have been tried and have failed. There are many such ideas within *A* which are left untried. Of course I can't guarantee that developing these ideas won't result in new theories all of whose extra content is empirically refuted. I also can't guarantee that the addition of some fundamentally new idea to *B* won't lead to its taking off again. But I can guarantee that this *will* require a fundamentally new idea. So, unless you feel pregnant with such an idea, start by trying to develop *A*'.)

Systems of theory-appraisal are then not entirely insignificant. Feyerabend is also wrong to suggest that a methodology which sticks to appraisal and refrains from issuing (rigid) advice is merely 'anarchism in disguise'. For anarchism, according to Feyerabend's own account, involves a good deal more than this. It allows, amongst other things, inconsistencies and claiming reliability for data which one knows to be unreliable. None of this is implied by restricting methodologies to appraisal. Nor, I would add, is any of it supported by Feyerabend's historical examples. For instance, Feyerabend points to very respectable scientific theories which were, at certain stages of their developments, formally inconsistent. But this on its own does not support the idea that inconsistent theories are rationally acceptable. Indeed if the desire to remove these inconsistencies provided part of the driving force of scientific development (as seems to be true in the cases Feyerabend mentions) then the principle of 'anything goes' in any novel interpretation is far from confirmed, it is refuted.

To sum up: Feyerabend's second argument rests on an interpretation of methodological rules which seems *a priori* much too strong to be tenable; he suggests that any methodology which resists this interpretation is empty, but this suggestion is incorrect.

In the course of his general argument against methodological rules, Feyerabend makes some important, more specific points. Often, however, their importance is obscured by the engaging rhetoric which accompanies them. What really amounts to good methodological commonsense is made to *sound* very challenging and new. While supporting Feyerabend's campaign against philosophical boredom, I find his jazzing up of rather ordinary theses often misleading. One example of this is his claim (see pp. 38–39) that a scientific theory's empirical content depends not just on the theory and the available experimental techniques but also on what rivals to the theory exist. This is made to sound like a revolutionary challenge to empiricist orthodoxy. It turns out to depend, however, on regarding *very* high-level statements (such as 'the Brownian particle is a perpetual motion machine of the second kind') as empirical. One can argue that it is difficult to characterise the directly observable statements, but, wherever the boundary is drawn, statements about *perpetual* motion will surely be outside it.

A second case where Feyerabend's exposition misleads is his defence of the claim that a theory's being inconsistent with some well-established fact may be a sign of its strength rather than its weakness. This claim again seems to me correct. Indeed it is an obvious, though nonetheless important, consequence of Duhem's point that a scientific theory will in general be formally inconsistent with a factual statement only when many auxiliary and observational assumptions are conjoined with it. Hence one may, in the event of a clash between theory and experiment, hold on to the theory and thus (since inconsistencies don't 'go'!) look to revise some auxiliary or observational assumption. If some such revision is successful, it will be regarded in turn as a great success for the central theory. In this way the development of new and better auxiliary and observational theories is stimulated. Many examples (such as Newton's 'correction' of Flamsteed's data which had seemed to refute his theory) have been extensively discussed.

This simple point seems, however, to have been widely misunderstood. This misunderstanding is likely to be increased by Feyerabend's formulation of the point, in which it becomes bound up with the question of 'theory-impregnation' of the facts. The facts at any stage in science's development are, according to Feyerabend, "constituted by older ideologies" (p. 55). It would therefore

> be extremely imprudent to let the evidence judge our theories directly and without any further ado. A straightforward and unqualified judgement of theories by 'facts' is bound to eliminate ideas *simply because they do not fit into the framework of some older cosmology* (p. 67).

Moreover, since this 'observational ideology' is presupposed in every factual statement, drastic measures are needed in order to criticise or test it:

> [T]he first step in our criticism of customary concepts and customary reactions is to step outside the circle . . . and either to invent a new conceptual system . . . that clashes with the most carefully established observational results and confounds the most plausible theoretical principles, or to import such a system from outside science, from religion, from mythology, or the ramblings of madmen (p. 68).

But are any of these worries about the difficulty of circumventing 'observational ideologies' justified by the examples Feyerabend gives?

Let's take Feyerabend's favourite example again. There are many ways of describing the result of the tower experiment. Some of these are highly 'theory-impregnated' (*e.g.* 'A body is released from the top of a tower which is travelling through absolute space at velocity v, and the body falls to the earth's surface under the sole influence of the gravitational force'). Other ways of describing the experiment are less theory 'impregnated', closer to being 'directly observational'.

Let's say that we take as our description of the outcome: 'A rock was released from the top of the tower and fell close to the base of the tower'. This is no doubt still fallible in many (not very interesting) ways (demons, hallucinations, *etc.*). But whether or not some 'observational ideology' is still involved in this statement, certainly its truth was never in dispute: both Galileo and his Aristotelian opponents agreed to it. What *was* in dispute was the 'interpretation' of this experimental outcome (as thus described). Feyerabend argues that Galileo had to replace one 'natural interpretation' (roughly that all motion is 'operative' or has observable effects) with another (which allowed for the possibility of the unobservability of relative motion). 'Natural interpretations' are discussed at great length and are somehow tied in with very wide-ranging world views, with the reality of our observations, with the psychological and physiological properties of our perceptual apparatus, *etc.* All of which is very interesting, but isn't the following account at least equally adequate whilst at the same time being simpler and less mysterious?

Galileo analysed the claim that the outcome of the tower experiment refuted the theory of the earth's diurnal rotation. He found that the claim relies on a further assumption (which had hitherto no doubt been only implicit) that the rock, when dropped, ceases to share in the rotation. He then pointed out that this extra assumption is by no means ungainsayable, and that there is at least some good evidence for the rival assumption.

On this account all the mystery about 'observational ideologies', about 'correcting' our sense data, goes and we are left with the old point of Duhem's – that auxiliary assumptions will be needed to draw from the 'theory under test' consequences which are genuinely decidable on the basis of observation.

One claim that Feyerabend makes when arguing for the 'anything goes' principle seems to me not just exaggerated but baseless. This is his claim that propaganda, unreason, is necessary even for what the rationalist sees as rational progress in science. Feyerabend supports this general claim by the specific historical claim that Galileo often knowingly used propagandist devices under the guise of rational arguments. (Of course, in

Feyerabend's scheme of things this is praiseworthy.) Feyerabend claims that Galileo's propaganda encouraged his contemporaries to accept ideas which they otherwise would have rejected. In particular, Galileo is alleged to have claimed reliability for the evidence provided by his telescopic observations when he knew this evidence to be unreliable. This telescopic evidence was used to defuse the objection to the heliostatic theory based on the fact that the apparent sizes of the planets Mars and Venus as observed with the naked eye do not change as much as they theoretically ought. It was also used positively to support Copernicanism (*e.g.* observation of Jupiter's moons).

I found Feyerabend's treatment of Galileo and the telescope the most fascinating part of Feyerabend's book. I learnt much from it. For example, one might have thought that Galileo's claim that the telescope was 'a superior and better sense' could be overwhelmingly supported by independently testable evidence. But, as Feyerabend points out, all this evidence was terrestrial. Moreover, as Feyerabend argues, the claim that the telescope may be terrestrially reliable and yet celestially unreliable is not so *ad hoc* as it may seem. Indeed the idea of a terrestrial/celestial difference here is immediately supported by the fact that terrestrial objects appear larger when viewed through the telescope, whereas stars, for example, appear smaller. Furthermore, there are, apparently, many aspects of Galileo's telescopic observation reports which are wildly wrong (*e.g.* his drawings of the moon's surface).

I am no Galileo scholar and so shall assume Feyerabend's historical facts to be accurate. How far do they support this general claim about the necessity for propaganda? Assume that Feyerabend is correct that Galileo lied about the reliability of this data for propaganda purposes. It would still, of course not follow that propaganda was here necessary for scientific progress. What evidence is there that the progress of the Copernican theory would have been any less impressive had Galileo been entirely open? Suppose that Galileo had said 'If the telescope is reliable then it gives great support to the Copernican theory. Moreover there is some evidence that it is reliable, but there are several empirical difficulties as well which I invite you to join me in working on'. Would the forward march of science have been interrupted? I doubt it. (Although as Feyerabend suggests, the forward march of Galileo's salary might have been interrupted!) Moreover the historical evidence here not only does not establish Feyerabend's general claim (which, of course it couldn't), it doesn't even support it. For Galileo only had one contemporary who contributed significantly to the development of the heliostatic theory, and that was Kepler. But, as Feyerabend himself points out, Kepler, far from being taken in by Galileo's propaganda, was extremely sceptical about the reliability of the telescopic results.

I said at the beginning that Feyerabend has three main arguments for his position. The third argument is based on his famous incommensurability thesis. Most of the 'rationalist' methodologies presently afloat presuppose that rival scientific theories can be compared for empirical content. This idea has already run into many severe, local difficulties, but Feyerabend's incommensurability argument threatened altogether to rule out all such content-comparisons.

Chapter 17, by far the longest in the book, is entirely given over to a development of the incommensurability thesis. I must say that I find Feyerabend's whole approach in this chapter uncongenial and unilluminating. It seems to mark Feyerabend's reconciliation with an earlier philosophical love: Wittgenstein.

Feyerabend's initial statement of the thesis seems to me incomprehensible. He writes: 'The content classes of certain theories are incomparable in the sense that none of the usual logical relations (inclusion, exclusion, overlap) can be said to hold between them' (p. 223). But, assuming 'exclusion' means 'no overlap' (*i.e.* empty intersection of content classes) surely these three categories are exhaustive?

But this is presumably just a slip. More important is the fact that Feyerabend then proceeds by claiming that grammatical habits or 'language games' (and their 'suspension') are what philosophers should analyse and not statements or propositions (and their refutations). He also claims that certain philosophical theses (by implication the most important and deepest) cannot be stated clearly, but only more or less vaguely felt. It turns out, for example, (p. 225) that

> As incommensurability . . . involves major conceptual changes it is hardly ever possible to give an explicit definition of it. Nor will the customary 'reconstructions' succeed in bringing it to the fore. The phenomenon must be shown, the reader must be led up to it by being confronted with a great variety of instances . . .

Moreover, Feyerabend decides to develop his thesis mostly in connection with styles of painting and drawing. His grounds for this are that, in the case of *theory* comparison,

> any debate of unusual ideas is at once stopped by a series of routine responses ... [And so the] best way to proceed in such circumstances is to use examples which are outside the range of the routine responses (pp. 229–230).

On the contrary, one would have thought that the best way to proceed would be by tackling the 'routine responses' head on and showing that they do not affect the thesis!

The switch to art provides the opportunity for a very enjoyable Feyerabendian *tour de force* covering aspects of literature and general cosmology as well as the visual arts, but it also obscures the issue. As a 'rationalist methodologist' I would find it disconcerting if it turned out that any two rival scientific theories are, despite appearances, necessarily incommensurable; but I am not at all disconcerted or even surprised to learn that styles of drawing and painting may be incommensurable. Indeed comparing, say, a Cubist painting of a woman with one by Leonardo da Vinci, this is precisely what one would expect.

Luckily Feyerabend does manage to make some clear *statements* about *scientific* incommensurability and it rather quickly turns out that the theory-comparer has a lot less to fear from the beast than earlier sightings may have indicated.

For example, any trace of 'psychological incommensurability' seems now to have disappeared. Earlier, at least in Kuhn (and Feyerabend seemed to support the idea), the claim was that scientists who have worked on incommensurable theories find it impossible to understand and communicate with one another. But now

> It is ... possible that being well acquainted with both [incommensurable] theories [scientists] change back and forth between them with such speed that they seem to remain within a single domain of discourse (p. 283).

And, more importantly, Feyerabend is now willing (or perhaps always was willing) to admit that

> Theories can be interpreted in different ways. They will be commensurable in some interpretations, incommensurable in others. Instrumentalism, for example, makes commensurable all those theories which are related to the same observation language and are interpreted on its basis (p. 279).

This, it turns out, is true of all the favourite examples such as the theories of Newton and Einstein. These two theories are commensurable if interpreted instrumentally, *i.e.* they are commensurable at the empirical level. Indeed what Feyerabend says on this point seems in the end to come dangerously close to the following triviality: We may say that Einstein's and Newton's theories make (possibly conflicting) statements about the same objects, but this is to interpret the two theories instrumentally. If we interpret it realistically then Newton's theory says something only about Newtonian objects (which, amongst other things, have, when involved in no physical interactions, constant spatial dimensions). It is thus entirely incommensurable with Einstein's theory which (when interpreted realistically) says something only about the entirely different class of Einsteinian objects (which, amongst other things, have velocity-dependent spatial dimensions).

Most of Feyerabend's rationalist opponents will, I suspect, be quite happy to admit that rival theories are incommensurable in this sense! After all, nothing prevents us from using the instrumentalist interpretation to tie the two theories to a common observational language in order to compare them. This move does not, of course, commit us to regarding the theories as *nothing but* instruments.

I have struggled hard to understand Feyerabend's general account of incommensurability and his explicit application of this general account to the particular case of Newton's and Einstein's theories. I am not at all sure that I have succeeded. One theoretical framework B is, it seems, incommensurable with another, A, if adopting B involves 'suspending' certain 'universal principles' (or rather 'grammatical habits') associated with A. Feyerabend stresses time and again that 'suspending' a principle is not the same as contradicting it. Much of Feyerabend's account leads one to believe that these 'universal principles' *must* be implicit – one can realise that one is making the assumption such a principle embodies only from outside the framework, the articulation of the assumption takes one outside the framework one was in before. These principles "involve something like a 'closure': there are things that cannot be said or 'discovered', without violating the principles (which does *not* mean contradicting them.) Say the things, make the discovery, and the principles are suspended" (p. 269).

Moreover, all the 'facts' that someone within framework A can see are constituted by these universal principles. Thus '[s]uspending universal principles means suspending all facts and all concepts' (*ibid.*).

One of the universal principles of Newtonian mechanics that was allegedly 'suspended' in the switch to Einsteinian mechanics was the principle that 'shapes, masses, periods are changed only by physical interactions' (p. 271). But why should the 'suspension' of this principle have any of the dire consequences for rationalism that Feyerabend envisages? Certainly a classical physicist will seek to find some force to explain any given changes in the spatial dimensions of a body. But the realisation that he is making an assumption here need not take him outside the classical framework. Indeed many classical physicists (Lorentz and Poincaré, for example) clearly entertained the possibility that an object's shape may be changed without its undergoing any physical interaction. They entertained the possibility, found it unacceptable, and tried to provide alternative explanations for various effects which seemed to show that this possibility was actualised. Moreover, I can't really see why we need this new notion of 'suspension' here. Surely Einsteinian mechanics entails that a body's shape is a function of its velocity and this simply *contradicts* the Newtonian assumption. While as for this principle being involved in 'constituting' classical facts, this claim again seems to rest on taking much too high-level statements as factual. To say that one arm of the Michelson interferometer was shortened simply because of its velocity through the ether may well involve 'suspending' certain classical assumptions (though again I don't really see why classical physics doesn't just contradict this statement). But the relativist and the classicist can easily agree on much lower-level facts (the shift, or absence of it, of the interference fringes) and can compare the ability of the two theories to explain *these* facts. Where is the significant incommensurability in all this?

I do not, of course, claim that there is nothing to be learned from Feyerabend's Chapter 17. I only doubt that any of it provides any difficulty for those whose aim it is to produce systems for the rational comparison of competing scientific theories. I should add that Feyerabend *does* provide elsewhere in the book some genuine though more local difficulties for the methodologist who would like to be able to say that in the various scientific revolutions the superseding theory had higher empirical content than the superseded theory. Here Feyerabend argues two specific points (the second of which was entirely new to me):

(1) Scientific revolutions often involve losses of empirical content as well as gains. This goes unrecognised, however, because the full power of the old superseded theory is forgotten.

(2) Often some (or even all) of the apparent excess content of the new theory is achieved only by the bogus method of '*ad hoc* approximations'.

I am quite ready to be convinced of the truth of point (1), but Feyerabend's examples are very unconvincing. We are told many times of the wonderful explanatory power of such systems as witchcraft, voodoo and Aristotelian physics. But we are never told precisely wherein this explanatory power resides. Of the examples cited, Aristotelian physics would seem to provide Feyerabend with the best opportunity to make good his claims. And certainly we are often told to "[r]emember that . . . Galileo drastically reduces the content of dynamics: Aristotelian dynamics was a general theory of change comprising locomotion, qualitative change, generation and corruption. Galileo's dynamics and its successors deal with locomotion only . . ." (pp. 160–161). But this is not enough to make Feyerabend's case. It has to be shown that Aristotelian dynamics provided *scientific explanations* of these extra facts. No doubt it did provide a 'world view' into which various facts from various different fields could be fitted, but this does not yet amount to a scientific explanation of these facts. No doubt, to take another of Feyerabend's examples, many psychological facts could be, and were, interpreted in terms of demonic possession, witchcraft, and the like, but Feyerabend nowhere convincingly demonstrates that any such fact was given a scientific explanation by any theory of witchcraft.

On the other hand, I found Feyerabend's second claim (about bogus increases in explanatory power) entirely convincing. The method of '*ad hoc* approximation', by which these bogus increases are achieved, consists in using a superseded theory up to a certain point in a calculation and then using the new theory (which is however inconsistent with the old) to 'refine' the prediction. (For details see pp. 61–64.) Feyerabend explains how this differs from legitimate uses of approximations, and warns that "*Ad hoc* approximations abound in modern mathematical physics" (p. 63). Clearly those who see the rationale for the various scientific revolutions in the increased explanatory power of the revolutionary theory will have to check that these increases were not achieved in this bogus way.

Paul Feyerabend's book is essential reading for all those interested in the problem of status of scientific knowledge. It will (I trust) win few serious converts, but non-anarchists will benefit from reading it because they will find in it much to challenge their own ideas. They will also find many fascinating snippets of historical information and comments on contemporary science, as well as the usual 'wicked asides' and amusing footnotes. But so far as its central negative arguments are concerned, it does seem to me that although 'rationalist methodology' does not escape from Feyerabend's attack entirely unscathed, it receives no mortal wounds. 'Method' lives!

Department of Philosophy, Logic and Scientific Method, London School of Economics

JOHN WORRALL

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