paul feyerabend's anarchistic epistemology

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In a series of brilliant and stimulating papers,Paul Feyerabend has demolished many rusty idols of the philosophical theater(1). His later works, and the book here under discussion (Against Method NLB, 1975) carry the fight into the scientific and military-industrial marketplace. Whereas he previously criticized certain approaches to the methodology of science, he now employs this critique to demystify the institutions and practices of the scientific establishment. His arguments in support of "theoretical anarchism" are certainly not limited to the physical sciences, and hence should be of interest to readers of this journal. As I'll explain, I feel that this book must be read, and am certain that it will be read.I am also certain that its virtues will go unnoticed, while its flaws will be extolled. Hence this review, dear reader!

Most philosophers have always wanted Science to "progress", but few anglo-american thinkers have suggested that this can best be achieved through the creation of a generalized spirit of creativity simultaneously throughout all cultural fields. And nobody before Feyerabend has suggested so forcefully that this more comprehensive job can best be carried out by explicitly abandoning the search for "methodological rules" to ensure, or make probable, such progress. Hence we are dealing with a philosopher of culture and not only with a philosopher of science. Herein lies a virtue: our author sees science as a complex system of social practices, jobs, boring or lively books, debates, controls, etc.: most other philosophers of science see it as a set of sets of sentences, wherein each set is a pristine codification of some theory at some historical stage of its development(2).
This conception of what a scientific theory is was developed in the heyday of the Vienna Circle, slowly refined by Carnap, Hempel, and others, and has come down to us as the Holy Writ of Logical Empiricism (which, for reason's I'll be glad to discuss elsewhere and elsewhen, is a better and more just name than is positivism, a word that fills people's hearts with transcendental loathing).Another Feyerabendian virtue is that he was perhaps the first to turn the principles of logical empiricism against itself, in order to show that other principles of the school were untenable. He has doubtless succeeded, to the endless dismay of the die-hards.
But Against Method is correllatively flawed. For its conclu-

sions and theses require the earlier arguments for their support, while the latter are not prominently displayed in the book. Acceptance of many of Feyerabend's points shall be induced by their current popularity, and by the excellent style in which they are presented: logic will be vetoed by passion, which is always bad, even when many of the points at stake are commendable.

Now our subject's main thesis is "anything goes". This seems to mean in part that the best procedure for the scientist, in presenting his or her work, is (a) to use forceful language and dubious argumentation which Feyerabend characterizes variously as "propaganda" or outright "trickery", (b) not to refrain from using several seemingly unsupported theories to support each other through creation of a seeming coherence: an old theory or world-view can be overthrown by an impressively coherent collection of theories, each of which is (at the time of the battle) inadequately supported. Thus a new "cosmology" can replace an old one. These two procedures go under the term "counter-induction", and are deemed desirable and necessary for the progress of science and of social institutions. Galileo is, for Feyerabend, the most impressive practitioner of these methods.

Let's look at (a). Galileo wished to support Copernicus and Kepler, who held that the Earth rotated both around the sun and around its north-south axis (both theses being in opposition to Ptolemaic astronomy(3). He had to argue these points, and he desired to convince others that they were essentially correct. But, said his opponnents ("Aristotelians"), if one drops a football from the Munttower and _if_ the Earth rotates on its axis during the drop, why is the ball not left behind; why does it fall at the foot of the tower, i.e. why does the ground not move under the ball while it falls?
A very reasonable counterargument, no? NO, says Galileo, for even all you benighted Aristotelians will surely admit that there are situations which are similar to that just described, and in which the observations to be expected if the Aristotelian argument were correct are not realized. Think of a baseball dropped from the mast of a moving ship, You all _know_ that the ball falls at the foot of the mast, and that it surely does not end up fore or aft (depending on the direction of the ship's motion: herein lies a problem!). Moreover, you ALL know, ALREADY, exactly why it drops as it does: for it shares in the motion of the ship, and while it indeed falls "downward", it ALSO moves horizontally, "keeping pace" with the ship's motion. So it falls, more or less, where we all expect it to. And now comes the punchline: why should objects dropped on a moving earth (space_ship_ Earth!) behave in some strange and different fashion? Indeed, it ought not to.
The peripatetic opponents of modern science, reason, and progress are immediately immobilized. Enter modern science.

This is a counterinductive procedure, since it employs an observational situation which was a paradigm case in support of an earlier theory or cosmology to support a new theory. Galileo wins by convincing his opponents that they ought to agree with him, since they "really" knew about relative motion already. (This method supposedly derives from Plato's technique of convincing slaves that they already know deep mathematical theorems since they can, in some sense, remember, or innately embody, the principles needed to prove them). The simple-minded Aristotelian need only (a) redescribe all of his observations in terms of relative motion of objects observed, (b) apply the same descriptive principles to the earth itself and to celestial objects. Thus one becomes convinced that there is nothing wrong in principle with admitting that we spin about, or at least that this central argument preventing the admission can be "defused".

Now this argument is approvingly characterized by our anarchistic author as "propaganda" and as "trickery", and the former it most certainly is. Strong language and forcefull bullying are used here, as you'll see if you read the original Galilean texts (which are at least as beautiful as the much praised Platonic Dialogues). But many scientists employ such techniques in their expositions(4), as do politicians, and there is nothing intrinsically bad or "irrational" in using such a demagogic style. A great playwrite (Brecht) convinces the audience of a moral or social thesis in the same fashion. So we are all propagandists at heart, at least whenever we take a thesis, life-style, or struggle seriously enough to worry about how to convince others of its virtues.

But trickery is something else. Indeed, it is quite misleading for Feyerabend to conflate propagandistic and fraudulant procedures. As I've outlined it, the argument is an <u>argument by analogy</u>, and is also, to a certain extent, inductive. Look. <u>If</u> the earth rotated about its axis, and <u>if</u> mundane objects rotated with it (as does the baseball with respect to the ship above: herein lies the analogical component) then (given, perhaps, some other premises) mundane objects will (one can deduce) behave as observed. Now reason "backwards". We indeed observe the above-described phenomena, and now, <u>by analogy with the ship</u>, <u>etc</u>. we grant the existence of relative motion. This lends support, to the first 'if' above, and helps to make the rotation hypothesis plausible.

Galileo had enough problems, at the hands of the most destructive of all our institutions, religion. Let's not give him more <u>tsores</u>.

No doubt, people will appropriate this snacky conflation, and will argue that just as in the Galileo-case, one <u>ought</u> to use (not only propaganda but) trickery, and that Feyerabend has indeed illustrated, by a glorious test case, the virtues of trickery. I don't want this lesson to be extracted from Feyerabend's brilliant text, since (a) I simply don't like to be fooled by anybody, no matter how lofty the cause (b) I fear, and I have seen in my teaching, that the virtues of propaganda *cum* argument get transformed into the evils of conceiving of science as a virtuously "irrational" institution. This needs expansion, so I must forego exposition of the second counterinductive procedure mentioned above.

Feyerabend has shown that there are some "rules of scientific method", concocted since Aristotle, that scienctists do not use in their work. He has also shown, along with Lakatos, that there are other rules (stemming from Popper) that they ought not to use, if they have certain *aims* that they pursue through their research. And there are further rules (e.g. those concerning how to construe the "reduction" of one theory to another(5)) which they cannot use if certain aims are to be realized. Many detailed arguments for these points are found in Feyerabend's earlier work, hence the incompleteness of *Against Method* deplored above. Thus, there is no scientific "cookbook", and the earlier philosophers of science were indeed seriously misguided in thinking that they could produce one. Theoretical Anarchism is, in part, the position that one ought not to use such pseudo-rules, and should stop looking for "real" ones. This certainly does not entail that trickery is in order, or that "irrationality" is a nice term with which to describe the practice of science. So certain aspects of Theoretical Anarchism that are appealing to me need support from earlier work.

How can science, or *a* science, or a *theory in* a science, be "rational", if its practitioners or producers do not "follow pregiven rules"? There are at least two interesting and true answers to this question, so I'll briefly discuss both: my essay here becomes a correction to Feyerabend, rather than a review.

First of all, consider certain products of the scientific mode of production: theories. There are other products, such as policies, devices, diplomas, etc., but we'll forget about these. Unfortunately, few of us ever get to produce a theory, but all normal scientists read and speak about them, and in some sense even use them. Likewise, philosophers of science can discuss them. All of this is true, although the question: what is a theory?, is not one that admits an easy answer. Nor, perhaps, should one expect a general answer laying down necessary and sufficient conditions for theoreticity. But we can and do talk about what we don't *fully* comprehend (yet!).

Now many philosophers and scientists are interested in these theories, and propose various ways of explicating the structure of given theories. This can be done without answering our general question. Any particular theory claims to "represent" a fragment of "reality", and philosophers try to illuminate the method of representation here postulated. The logical empiricists claimed that a theory (e.g. relativity theory) _is_ a certain set of sentences, or can be best thought of as such a set (p. 1 of my text). The difficulties with this view have resulted in a new concept of a "theory of mathematical physics", due to Sneed(6), in which the fragment of reality in question is describedwithin an extension of set theory. The details are not important here, although Sneed's work does give some insight into the "dynamic" problems involved in describing the "growth" of a physical theory: the new conception's application to things that claim to be social-scientific theories is still to be tested.

So let's assume that certain more or less articulated views of certain thinkers can be called theories, and that each of these submits to some precise form of exposition. The history of philosophy of science shows that this can be done. Then questions _about_ these theories (not about how they are produced, discovered, used to emancipate us!) can be precisely transformed into questions about the exposition of the theory (which might employ techniques of mathematical logic and/or set theory; see above). Many of us find some of these questions interesting _in their own right_. What does General Relativity tell us about time? Why is statistical mechanics a worthy "replacement" for traditional thermodynamics? What is the precise concept of "drive" employed in some psychological theories? Why is no reasonable democratic voting system possible? All of these questions can be made precise by "translating" them into the language of some exact (i.e. logically articulate) exposition of the theory in question, and can, perhaps, be answered unequivocally. Thus "science" is rational in the sense that some theories can be rationally reconstructed(to use professional jargon), and questions about the theories can be precisely stated and answered.

But please don't misunderstand me. I am not stating that this can always be done (with respect to any given body of claims that somebody calls a theory), and I'm not stating that anything of value will _ipso facto_ be gained even if it is done. Each case must be explored for its own interest(7). Indeed Feyerabend feels (see the note on p. 155 of his text) that such reconstruction tells us little or nothing about the nature of scientific progress. I'm just claiming that certain scientific products can be coherently, i.e. rationally, expounded and discussed, and that philosophers of science have developed tools to facilitate this. Surely no _one_ toolbox should be expected to provide everything, and should not be condemned for this inability. Feyerabend of course does not go to such inane extremes, but again, I'm afraid that many of his readers will.

Now for the mind's second road to rationality. The denial, by Feyerabend, of pregiven universally applicable methodological rules of discovery or confirmation, is surely in order. But this DOES NOT imply that any given instance of scientific inquiry (be it experimental or theoretical) need be methodless in some interesting and "deep" sense. It is probably both true and trivial to say that any given act of research, by a person or by a "community", employs methods that are "rational" given the aims of the actors. This does not mean that these methods are found by consulting our much-maligned cookbook. It also does not mean that one ought, if necessary, to resort to "trickery". I certainly do not have a definition of what a method is, or of what it is to be methodical, although I think I'd like to have these. But I do think that case studies would show that the procedures of workers in particular casesof inquiry were methodical, and that you and I would agree on this in most cases. Again, I certainly do not claim that no scientific worker was ever, in some sense, unreasonable.

Given the above, one may argue that there is some interesting correlation between the aims of a given researcher, and the methodological tools that will be employed in the conduct of a given piece of research. One's interest (for the benefit of all the Habermas fans in my audience) guides one's choice of tools, and these differ from research-occasion to research-occasion. This view leads to a revision of the history of science (and of philosophy), which is one thing Feyerabend wishes to bring about. He has definitively convinced us that we are misguided if we think that scientists have in fact proceeded by deliberately applying some item from the eternal toolbox.Being no expert in the history of science, I can't say to what extent scientific work has been misrepresented before Feyerabend and others placed us on the secure path of a historical science. But as a philosopher, I can say that he has shown that the use of certain preconceived philosophical conceptualizations of scientific change has resulted in a distorted picture of what a theory is and of what a progressive theory-change is. Let's be thankful for what we have, and not (mis)represent Against Method as the new Holy Writ.

To end, I'll retreat to some claims in my second paragraph. Our author feels that the same noncommittment to rules can be beneficial to progress in all cultural fields. He also seems to hold, quite reasonably, that "science" is but one aspect of a "culture", i.e. one cultural formation "within" a "system" of such, and is thus interwoven with all aspects of our social structure. Such a view of a society is already enough to deflate any high-sounding claims about the a priori superiority of science (say, as compared to art) as a force determining, helping, and controlling, aspects of our lives.

It also goes some way towards a sorely needed argument that our lives will not necessarily be qualitatively improved if scientific projects are given societal priority over projects of other sorts (e.g. artistic). That is, other institutions might be at least as worthy of support as those that propagandistically claim to have produced mankind's most helpful productions. For if one aim of human cultural activity is to relieve us of our drudgery, than fruitful results might be achieved by the application of "anarchistic" approaches in many cultural institutions(8).

This realization may awaken a pervasive spirit of creativity in society, whose consequences can be invaluable. First of all, the fetishistic aspects of previous institutional forms will be made visible. These aspects(e.g. the belief that a certain institution should be managed as if its progress were all-important, or the belief that scientific institutions are of supreme value precisely because their members use the supremely valuable method) have generally outlived their usefulness. So reading Feyerabend's attempted exposure of certain myths of the scientific establishment (which are supported by some of its philosophical mandarins) can open our eyes to similar myths elsewhere.If Feyerabend accomplishes this, than it matters little if his specific attack upon the historiographic tradition is successful. I therefore feel that one ought to read this book, and ought not to be put off by his use of examples from the natural sciences.

I hope that people will approach the text in the spirit that I'm suggesting. I hope also that they'll think of the social issues involved, not all of which are raised in the text. Most importantly, what social systems both permit the practice of theoretical (read: cultural) anarchism and put its results to the best use? It is clear to me that capitalistic systems can hardly do so, given their profitpriorities: the latter encourage just those fetishisms that Feyerabend wishes to expose and destroy. It is also clear to me that institutional frameworks must be established which are humane and within which theoretical anarchism can be practiced (assuming that it really is a desirable praxis). Proposals concerning the forms of these new frameworks can responsibly arise only from a critique of present political and economic constraints facing the establishment of these frameworks. Discussions of outmoded or desirable scientific institutions must be embedded in discussion of outmoded or desirable social systems in general; i.e. in a discussion of procedures for the dissolution of outmoded capitalistic institutions and for the construction of the socialist alternative.

Notes.

(1) My article discusses only a few aspects of Feyerabend's thought. Other topics that deserve discussion (e.g. the problem over the "incommensurability" of theories) are suppressed. See the comprehensive Bibliografie van Paul K. Feyerabend, compiled by Alice ter Meulen in November 1974. It is published by the Centrale Interfaculteit, Universiteit van Amsterdam.

(2) Wolfgang Stegmüller's multi-volume Probleme und Resultate der Wissenschaftstheorie und Analytischen Philosophie contains an elegant review of aspects of this account.

(3) The best semi-popular study of the tradition we're discussing is E.J.Dijksterhuis' The Mechanization of the World Picture, Oxford, 1961.

(4) The relevance of this point to the present context was pointed out to me by L.Boon in conservation.

(5) See Feyerabend's "Explanation, Reduction, and Empiricism", in Minnesota Studies in the Philosophy of Science, vol III, ed. by H.Feigl and G.Maxwell, Minneapolis: University of Minnesota Press, 1962.

(6) J.Sneed, The Logical Structure of Mathematical Physics, Dordrecht: D.Reidel, 1971.

(7) I rely again on a conversation with L.Boon.

(8) A more exact analysis of these claims would require a distinction between institutions that facilitate pure science and those that facilitate applied science.