# Ideological Commitments in the Philosophy of Science

**Jerry Ravetz** 

Jerry Ravetz and Roy Edgley, Ideological Commitments in the Philosophy of Science With a Comment on Ravetz by Edgley

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To outward appearances the academic discipline of 'the philosophy of science' has in recent times been an austere and abstract study. Its concerns have been with one major problem, to the near exclusion of all others. The truthclaims of completed scientific knowledge have been considered to be the only area of really worthwhile philosophical inquiry. The process of discovery, or the ethical problems of research, or of applications, have often been relegated to the status of non-problems or at best peripheral ones. Even now, as these other sorts of problems gain in interest among philosophers, the absence of a coherent framework of ideas for constructive study inhibits their development; while epistemology, the theory of scientific knowledge, still dominates teaching because it at least provides materials that can be taught <1>.

Furthermore, the sort of science considered worthy of study is very special. So special in fact, that it might not even exist. For the main tradition in the philosophy of science, including its variants and critics, has been devoted to considerations of matured 'exact' sciences, in which quantitative experiments and mathematical laws combine, to give the most assured knowledge to which humankind can attain. Other sorts of disciplines are deemed 'immature'; and their main assigned task is to find ways to approach or achieve the proper state. The obvious paradigm case for a genuine science is physics, whose solidity is attested by its triumphs both in theory and in application. It has been noticed that the theoretical end of physics has been in a state of continuous conceptual turmoil and revolution for nearly all of this century, and so its own credentials as a steady, perfected matured science are not beyond criticism <2>. However, practitioners and defenders of this philosophical tradition can argue that even if this philosophy of science describes no actual science, it tells what any genuine science must be like. Its claims to special and unique status as a philosophical inquiry are not therefore dependent on whether its objects of study are precisely reflected in the imperfect world of human experience.

Such a conception of itself is quite legitimate for an academic discipline, particularly a philosophical inquiry. We do not ask geometers to go about measuring the earth, so we should allow philosophers of science a corresponding freedom to develop their own autonomous discipline. It is unfortunate that some people so misinterpret the field as to try to glean insights from it about the status and methods of confessedly immature descriptive sciences  $\langle 3 \rangle$ ; but that cannot be the responsibility of philosophers. The philosophical task of showing how assured human knowledge can in principle be obtained in some sorts of natural science, is one that takes priority over merely practical

concerns.

If all the foregoing argument for purity seems as reasonable as I have tried to make it, we are well prepared for an historical paradox. This is, that many of the main protagonists in the development of twentieth-century philosophy of science have been deeply committed to causes directly involving humanity; and their doctrines of the philosophy of science were shaped with those broader ends consciously in view. The reason that 'science' in this tradition seems unlike ordinary practice is not because of its being a purified object of abstract conceptual analysis, but because of its being a symbol of the Good and the True in a certain ideologically engaged tradition of philosophical polemic. If, as I believe, it is time to move on beyond the insights and scholarly problems of that tradition, we should appreciate its sources of commitment so as to make an accurate and sympathetic assessment of its permanent achievements. Also, we will be better able to understand its particular weaknesses and thereby to remedy them in our own studies.

#### The Vienna Circle: Proclaiming the True in Science

The focal point of the coherent tradition of philosophy of science was Vienna during the 1920s and early 1930s. There flourished the 'Vienna Circle', a grouping of philosophers and other scholars which included Karl Popper on its periphery. While Popper's writings, philosophical and autobiographical, are clear on his deep and abiding political commitments, the better-known English language writings of the members of the Vienna Circle do not overtly depict such influence. However, a recently published translation of a manifesto issued in 1929 over the names of the members, makes it plain that it saw itself as participating in a tradition extending back to the Enlightenment. Here we find the struggle against 'dogma and metaphysics' (the intellectual tools of the reactionary clerical forces) emphasised, as well as the invocation of 'science' as the unique way to truth and human improvement:

'The increase of metaphysical and theologising leanings which shows itself today in many associations and sects, in books and journals, in talks and university lectures, seems to be based on the fierce social and economic struggles of the present: one group of combatants, holding fast to traditional social forms, cultivates traditional attitudes of metaphysics and theology whose content has long since been superseded; while the other group, especially in central Europe, faces modern times, rejects those views and takes its stand on the ground of empirical science. This development is connected

with that of the modern process of production which is becoming ever more rigorously mechanised and leaves ever less room for metaphysical ideas. It is also connected with the disappointment of broad masses of people with the attitude of those who preach traditional metaphysical and theological doctrines. So it is that in many countries the masses now reject these doctrines much more consciously than ever before, and along with their socialist attitudes tend to lean towards a down-to-earth empiricist view. In previous times, materialism was the expression of this view; meanwhile, however, modern empiricism has shed a number of inadequacies and has taken a strong shape in the scientific worldconception. Thus, the scientific world-conception is close to the life of the present. Certainly it is threatened with hard struggles and hostility. Nevertheless there are many who do not despair but, in view of the present social situation, look forward with hope to the course of events to come. Cfcourse, not every single adherent of the scientific /orld-conception will be a fighter. Some, glad of solitude, will lead a withdrawn existence on the icy slopes of logic; some may even disdain mingling with the masses and regret the 'trivialised' form that these matters inevitably take on spreading. However, their achievements too will take a place among the historic developments. We witness the spirit of the scientific world-conception penetrating in growing measure the forms of personal and public life, in education, upbringing, architecture, and the shaping of economic and social life according to rational principles. The scientific world-conception serves life, and life receives it' <4>.

In support of this interpretation of the orientation and commitments of the Vienna Circle, we have the personal testimony of the Norwegian social philosopher Arne Naess. He recalled,

'The Vienna Circle was a nucleus of a movement for "rationality" and against certain forms of metaphysics which at the time were closely allied with fascism and national socialism. It had all the missionary zeal of a movement, and it was touching but also somewhat alarming to watch Otto Neurath embrace aloof and aristocratic Polish logicians of various philosophical affiliations and proclaim, "We agree! You are one of us!" If Neurath sensed that one was <u>somehow</u> on the right side, one was identified as a sort of logical positivist. Protestations were of little use and disagreements were conceived as due only to "unhappy formulations" (<u>unglückliche Formulierungen</u>) and there was always a remedy for that'  $\langle 5 \rangle$ .

There is a stylistic feature <6> of the Vienna Circle's studies which supports the interpretation of their being prophets in analysts' clothing. For their vision of science was quite deliberately abstracted from the processes of personal creation and historical development; and in this regard they were more extreme in their demarcations than their predecessor, Ernst Mach. For in his own critical studies, as of mechanics <7>, Mach allowed for the maturing of a discipline through several phases; the earlier, anthropomorphic ones as important and valid in their own way as those which were appropriate to a more perfected state. The Vienna Circle showed no interest in such origins or their vestiges, being concerned solely with the establishment of the credentials of statements in fully matured science.

Why these aspects of the Vienna Circle's programme have not been made prominent is a matter beyond my present purposes to explain fully. Let it suffice that with the rise of Nazism in central Europe, the surviving members of the school dispersed to the Anglo-American cultural area. There, the ideological battles were in a totally different style and on different issues. Given their new situation, it was understandable that the apparent content of the scholarly work should be emphasised and its ideological commitments (themselves severely shaken by the defeat of the anti-Nazi forces) left in discreet obscurity. Only now, when an avowedly ideological attack has been mounted on the very foundations of the positivistic programme, does this broader commitment again emerge explicitly.



Popper: Rescuing the Good in Science

In the case of Karl Popper, the clues to ideological commitment are available in one of his best-known works, Conjectures and Refutations. In a classic autobiographical essay, he describes how he came to conceive of the criterion of *falsifiability* in the demarcation of genuine science from its spurious imitations. Even allowing for the inevitable rationalisation in the recollection of an event after a lapse of nearly four decades, the story has all the intensity and drama of a genuine conversion experience <8>. Put simply, in 1919 the young Popper was a radical student who was inspired by four great thinkers who styled themselves 'scientists': Karl Marx mainly (Popper regarded himself during this period as a Communist), Sigmund Freud, Alfred Adler and Albert Einstein <9>. After the defeat of the Central Powers in 1918, the way seemed open for the forces of scientific rationalism to achieve their goals in society as well as in nature. But things began to go wrong: failures and complications in the political struggle, doubts and confusions in the intellectual debate.

Popper began to sense that the pretensions to 'scientific' status (meaning, that is, embodying the good and the true) of Marxism, psychoanalysis and individual psychology were not correct. Yet by the accepted criteria of the time, they were indubitably scientific. An adherent of Marx or of Freud could display numerous confirmations of their theories (very close to the principle of 'verification' that was at the heart of the Vienna Circle positivism). And Adler relied on the inductive evidence of his clinical experience for the development of his theories.

What strikes me as one of the most fateful instants in the philosophical thought of the century occurred when Popper queried one of Adler's instant diagnoses, and was assured of the psychologist's 'thousand-fold experience' of such cases. Popper reports that he could not help saying 'And in this case, I suppose, your experience has become thousand-and-one-fold' <10>. This could be read as a sarcastic little joke; but actually it sends a searchlight beam into the weak centre of straightforward inductive reasoning.

So Popper saw that the ordinary criterion used to distinguish real science from obvious pseudo-sciences such as astrology, its 'inductive' character, was incorrect; this did not capture its essence. Worse yet, once the practitioners of such pseudo-sciences were confident of their status, they could then use them in a particularly insidious fashion to insulate them from criticism. The critical Marxist is deemed 'petty-bourgeois', the sceptical patient is diagnosed as 'deeply neurotic' and so on. Thus immunised against criticism, such essentially speculative, non-specific studies could become really pernicious pseudo-sciences. (This point is not developed very explicitly in Popper's own account but it is worth emphasising, as it shows he had two distinct criteria for a pseudo-science: that which merely confirms theories and that which discredits criticism.) In this respect these supposedly 'liberating' sciences take on the worst feature of traditional theology; damning all disagreement as heresy. (I am indebted to Dr R. Sinsheimer, of the University of California, Santa Cruz, for this observation.)

Where, then, to find an example and a criterion for real science? Einstein's bold theory of general relativity, and, more, his dramatic challenge to the astronomers to test it in the eclipse of 1919 provided the experience. For Einstein had argued that Isaac Newton had been wrong on a fundamental point of his system of the world. And now he was calmly inviting the scientists to test his claim, to determine whether he was more correct than Newton -- or himself only the author of a misconceived theory. That was real science - not fake confirmations entrenched in dogmatism, but bold conjectures ruthlessly put to the test. Popper concluded that what made a theory scientific was not that it was verifiable, but that it was falsifiable. But the heart of his insight was that what differentiated a real scientist from a fraud was the moral quality of daring to be shown to be wrong.

This is a very deep insight into the essentials of our science and indeed of our modern European civilisation. If there is any doubt as to Popper's political commitment in its development, that can be removed by acquaintance with his influential works in political philosophy, The Poverty of Historicism and The Open Society and its Enemies, which emerged between 1935 and 1943. The achievement had its own cost, reflected in Popper's use of the 'falsifiability' principle in the philosophy of science. For Popper was not content to leave it as an essentially ethical principle of genuine scientific behaviour; he needed to adapt it to function as a principle of epistemology and of method <11>. Severe problems were then encountered, for it turned out to be exceedingly difficult to demonstrate how knowledge could increase as a result of applying tests designed to falsify hypotheses: if such a test was successful we gained only the knowledge that some particular statement is false; while if it was unsuccessful we learned only that the statement was not yet proved false. As a principle of method, the projection of bold, very general hypotheses is not even a good caricature of the way scientists work. And, as an historic joke of the sort frequently associated with Einstein, the astronomical observations he suggested would not have been admitted by himself as a refutation of his theory even if they had gone against it <12>.

The contemporary student derives from Popper's work a sense of urgency and commitment, something absent in the technical writings of the Vienna Circle philosophers. It is not made clear what the urgency is precisely about, since the scheme of 'science' portrayed there is arguably unlike the practice of either the ordinary or the great scientists. But with the help of the autobiographical essay and the political writings, we can appreciate the kinship of Popper to the Vienna Circle, both participating in the tradition of Central European rationalism, in which 'science' was not so much a particular social activity as a cause. However, we should recall the strong difference between them. Whereas the Vienna Circle proclaimed the good news of True Science in a thoroughly traditional Enlightenment way, Popper jettisoned the True of Science in order to rescue the Good. Post-Popperian philosophy of science may be seen as a test of whether even this desperate measure would suffice for the ideological defence of Science in the later, troubled years of the twentieth century.

In the history of ideas, time does not run at all smoothly. The matured programme of the Vienna Circle was developed after the revolution in 'atomic' physics was well

under way, and also after the insolubility of the 'foundations crisis' of mathematics had been proved by the most rigorous of mathematical arguments. Hence its confidence in the security and intelligibility of matured exact natural science was betrayed by events even before it became the basis of a programme. With Popper, time played other tricks: his insights of 1919 waited some fifteen years before appearing in print; and by the early 1930s the German language market for politically-liberal philosophy of science was drying up rapidly. So he spent long years in New Zealand preparing his political philosophy, on the basis of which he came to London. Only in the later 1950s, nearly forty years after the initial experience, did his philosophy of science begin to affect English-language academic opinion  $\langle 13 \rangle$ . It is a mark of the quality of this philosophy of science that it survived still fresh and stimulating; the long reign of the Vienna Circle philosophers and their associates and students was at last being challenged. Popper also had the pleasure of seeing a school develop around himself. But, inevitably, there soon appeared a threatening and in some respects sinister rival philosophy: that of Thomas Kuhn.

#### Kuhn: Kicking Open Pandora's Box

The enormous influence of Kuhn's work is partly due to the fact that he seems to be describing science the way it really is, and doing so in a manner which combined extensive historical knowledge and reflective personal experience. His scientists are neither the impeccable truthgatherers of the positivist tradition, nor the heroic conjecturalists of Popper, nor yet the paradox-generators of Lakatos. They are, normally, just ordinary people concerned only to solve research puzzles within an unquestioned framework of concepts and methods. Kuhn's own experience of science was in postwar America, where ideological struggles were very muted and science was well on the way to becoming big business. His account, reaching its audience when a rapidly expanded world of science and science education had lost most of its earlier sense of adventure and commitment, reads like the plain unvarnished truth. Because of this close relation to a new, disenchanted commonsense of science, its ideological significance is more difficult to discern and also more devastating.

According to Kuhn, scientific progress alternates vetween 'normal' and 'revolutionary' phases, in which (respectively) scientists make piecemeal advances, or choose between rival grand systems. By his account, it appears that normal science is boring, and revolutionary science incomprehensible. He offers no methods or criteria for helping scientists decide in a revolutionary situation. Hence the genuine 'progress' of science (so vital for its traditional ideological message) becomes impossible to account for, and hence to guarantee, in 'revolutionary' and 'normal' science alike. Indeed, Kuhn eventually reflected on the way that ultimate purposes are implicit in our idea of scientific 'progress', and wondered whether we couldn't dispense with it in the evolution of human knowledge just as we have done in the evolution of the species  $\langle 14\rangle$ . With disarming candour, he describes normal scientific work as 'the strenuous and devoted effort to force Nature into the conceptual boxes provided by professional education' <15>.

Having casually dropped the True, he equally lightheartedly dismissed the Good of Science. In his general account of the argument of his book he describes the response of established scientists to the crisis that precedes a revolution in such unflattering terms as the following:

'Normal science, for example, often suppresses fundamental novelties because they are necessarily subversive of its basic commitments ... when ... the profession can no longer evade anomalies that subvert the existing tradition of scientific practice then begin at last the extraordinary investigations that lead the profession at last to a new set of commitments, a new basis for the practice of science.' <16> Popper did well to entitle his own criticism of Kuhn 'Normal Science and its Dangers' <17>.

The most striking evidence as to what was <u>not</u> worrying Kuhn comes from an exchange of the mid-sixties, when the mischievous Paul Feyerabend observed that Kuhn's idea of 'normal science' as 'puzzle-solving within paradigms' provided no means of distinguishing between scientific research and other activities, even including organised crime <18>. The point of this remark was that the association of science with any sort of ethical consideration (either in goals or in methods) was completely obliterated on Kuhn's model. Kuhn's response was simply to remark that he never claimed that his model applied exclusively to science <19>. And there the matter rested.

Kuhn's work is an illuminating example of the way in which a doctrine can have ideological consequences in near independence of the purported concerns and commitments of the author. It could be, and was, used for a denial of an objective, universal basis of scientific knowledge, for several purposes. For instance, it seemed to offer a behaviouristic criterion for the genuineness of a scientific field: one where debate on fundamentals is irrelevant, and all work proceeds as puzzle-solving within a dogmatic framework. For insecure scientists in fields of human behaviour, this offered a justification of arbitrarily imposed conformity. But rebellious researchers and students could utilise the relativity of 'paradigms' to struggle for a substitution of their favoured dogma against the officially sanctioned one. Both sorts of move are destructive of the open dialogue which is the essence of Western liberal democracy, of which 'science' had for generations been taken by its advocates as the great exemplar. Hence for those with a strong sensitivity to ideology, Kuhn's doctrines were a menace.

#### Lakatos

Because of his early death the Hungarian Imre Lakatos achieved only a modest bulk of publications; moreover, his various papers are either difficult or controversial or both. Like Wittgenstein, the personality of Lakatos seems an important part of his influence; by his intensity, brilliance, and wit, he kept the spirit of Popperian committed philosophy alive, and he was also quite clear and explicit about his <u>own</u> ideological engagements.

From his student days onwards, Lakatos had been, successively: a member of the anti-Nazi underground; a Communist Party activist; a bureaucrat in the Hungarian State cultural apparatus; a minor victim of the Stalinist purges of the early 1950s; a candidate for a treason-trial, whose name happened not to be reached; a non-rehabilitated (therefore document-less) ex-prisoner in pre-liberalised Hungary; a rehabilitated person, student and member of the Petöfi circle during the Hungarian 'spring' of 1955 and 1956; a refugee after the Russian intervention of 1956; a research student at Cambridge, completing a thesis on the philosophy of mathematics; eventually a member of the Popper group at the London School of Economics; and finally an embattled opponent of the 'new left' student revolutionaries who concentrated on the LSE in 1966.

As Lakatos made clear in his published writing  $\langle 20 \rangle$ , the issue was plain: the defence of reason against its enemies, which (as Popper saw before him) could come equally well from the 'Left' as from the old Right. But, working so much later than Popper and endowed with greater self-awareness and subtlety in politics and philosophy, he could appreciate those defects in Popper's system which required remedying. This work, undertaken directly as a response to the challenge of T.S. Kuhn's book, <u>The Structure of Scientific Revolutions</u>, occupied the last years of his life and was of uneven quality. His earliest work, undertaken before he came under the direct influence of Popper, is more original and probably more significant. Its ideological commitments are not so open, but are thereby all the more worthwhile to explore.

Proofs and Refutations <21> is an essay in the philosophy of mathematics, in my opinion the first really new move in that field in the twentieth century. Previously, philosophers and mathematicians had attempted to resolve the 'foundations crisis' in terms of mathematics being a fixed and rigid intellectual structure, consisting of clear concepts linked by unambiguous rules of interference. The various foundational programmes were devoted to exposing that structure in such a way as to eliminate the paradoxes and anomalies that had been discovered there. Lakatos saw a very different problem: as a preliminary to any genuine philosophy of mathematics, we must explore the dialectic of development both of mathematical concepts and of criteria of rigorous proof. For both these are historically conditioned, and any philosophy that ignores this fact perpetuates the bad tradition of dogmatism in mathematical thinking. His method was as radical an innovation as his doctrine: he expounded his philosophy through a classroom discussion of terrifyingly clever schoolboys, dissecting their hapless teacher's proof of a classic result in topology, the 'Euler Polyhedron Theorem'.

The roots of Lakatos' philosophy of mathematics are clear: the strong Hungarian tradition of problem-solving mathematics, raised to an art and philosophy by G. Polya <22>; and a playful Hegelian style of dialectic, derived from a Marxism purified of its political content. His commitment was not so clear at the time of first publication of <u>Proofs and Refutations</u> in 1963-4; but it may be inferred from his life's work. One can imagine that the demonstration of the falsity of rigid and dogmatic thinking in mathematics, the most abstract of all sciences, could be applied <u>a fortiori</u> to the 'science of society' under which Marxist socialism was supposed to be constructed.

It could even be that Lakatos' philosophy of mathematics was among the most significant intellectual achievements of the 'Petöfi Circle' of the Hungarian Spring of 1955/6.

There is even a conjecture that his criticism of 'proof' was born as a survival-strategy under conditions of interrogation in Stalinist Hungary. We recall the game played in Koestler's <u>Darkness At Noon</u>, where the accused Rubashov had to admit guilt on any crime which he might logically have committed, given his other actions and beliefs. In that game, it mattered not that the particular accusations were, in a non-political factual sense, false. We may imagine that for an experienced person undergoing such interrogation, the prime task was to prevent the interrogator from convincing him that entering such a 'confession' game was a personal duty that could be rigorously derived from the objective needs of Party and Revolution. A denial of the irresistible cogency of even mathematical proof could, under those circumstances, provide an escape hatch from the exigencies of that political logic. (I am indebted to June Goodfield for this observation.)

The affinity in spirit and commitment between Popper and Lakatos is plain, at least <u>a posteriori</u>. They came together not long after Lakatos settled in England, and then jointly faced the challenge of the ideological consequences of a totally non-ideological philosophy of science. This was the theory of 'paradigms' in Kuhn's <u>Structure of</u> Scientific Revolutions.

Through the later 1960s Lakatos attempted to combat Kuhnism at the philosophical level. The great monument to this effort is the report on a symposium in 1965 <23>. He saw that the versions of methodology that can be read out of Popper's writings are all too naive to stand scrutiny; there could be no 'instant rationality' in scientific choice. His task was to construct a 'heuristic' that would allow both for the complexity of the cognitive problems (where testing of theories could be neither immediate nor decisive) and for the human qualities of scientists (rightly unwilling to throw away years of work at the sight of the first unresolved problem) while yet preserving the ethical and political commitments of Popper. His philosophical keenness led him into further problems (conveniently overlooked by most of his contemporaries) including the relations between the history and the philosophy of science, and also the location of the ultimate source of correctness of accounts of science (he put it in the successful practice, as distinct from the theorising of the <u>élite</u> scientists). The resulting edifice of ideas, further enriched by Lakatos' delight in polemic and paradox, was impressive but unwieldy. It was also very vulnerable to criticism in respect both of its historical reconstructions and its philosophical generalisations. And Lakatos, like Popper, failed to face up to the political consequences of his philosophical critique: if the dominant self-consciousness of science, enforced by its <u>élites</u>, has been false, reactionary, and dogmatic, what should we conclude about science as a social institution?

Lakatos did not engage in these exercises for their own sakes; while he was elaborating on his thesis he was also engaged in a political struggle with antagonists he considered as vicious and as dangerous as the Stalinist thoughtpolice of Hungary. The rebellious students of the London School of Economics in the late 1960s were, in retrospect, a small and ineffective minority. But during their flourishing, they disrupted a distinguished educational institution and announced their intention to destroy it and much else beside. Even the native English academic staff at the LSE were caught up in violent struggles, ideological, institutional and personal. For Lakatos, it was the Red Fascists on the march again, and he reacted, as if back in Budapest. This struggle convinced him that his version of Popperian liberal philosophy of science was central to the defence of civilisation, and so gave his work a compelling intensity. But it took a heavy toll of his energies, and left him exhausted and ill.

It is conceivable that he eventually recognised that the great flexibility he built into his model of rational scientific behaviour effectively undermined his political commitment. The crucial point is of time-scale; as he said, 'to give a stern "refutable interpretation" to a fledgling version of a programme is a dangerous methodological cruelty. ... (it) may take decades of theoretical work to arrive at the first novel facts and still more time to arrive at interestingly testable versions...' <24>. Decades, for an abstract scientific theory? What then, for a new social system? By this criterion, the Soviet intervention of 1956 was quite possibly 'historically necessary' to protect the fledgling Socialism of Hungary; and Lakatos' lifelong exile perhaps the result of an error in assessment of an early version of a social-development programme. Only an intimate biography could tell whether Lakatos appreciated this latent contradiction; but I think its logical consequences for the deduction of political philosophy from such a scientific methodology are inescapable.

#### Paul Feyerabend and the End of Classical Viennese Philosophy of Science

Feyerabend is certainly the most confusing and paradoxical figure in contemporary philosophy of science. It is not at all easy to decide whether he is a court jester, Zen master, or Fascist. The first, because he still operates within the community of philosophy of science, engaging successfully in highly technical debates on problems within the dominant style. In this respect, he is more one of the club than even Lakatos ever was, to say nothing of Kuhn, whose real commitment is to interpretative history rather than exemplified philosophy. Conventional philosophers of science cannot dismiss him, for he is capable of publishing a fully expert and illuminating - or wounding - study of problems or per-sons at any time. Yet he writes wild and reckless destructive criticisms of the whole programme of mainstream philosophy of science, the explanation and justification of the methods whereby scientists gain new knowledge. Some might hope to contain his influence by not taking the critical diatribes too seriously, and treating him as a court jester, who says impossible things as useful reminders of the human frailties to which even philosophers are subject.

Careful consideration of his arguments shows that they are not so easily reduced to jokes. If philosophy of science has any pretensions to help us understand the activity of science, then his studies of the behaviour of great scientists are troubling indeed. For he shows by example that for any explicit rule of method enunciated by philosophers of science, there is an important occasion on which it was broken by some great scientist. In his Against Method <25> he goes far towards demonstrating that Galileo was a precursor of Feyerabend, treating all the rules, including that of simple accuracy (or honesty) in recording observations, with fine anarchistic playfulness. The epoch-making description of the surface of the moon that Galileo saw through his telescope, reported in the Starry Messenger, gives prominent and important reference to a feature (a large round - crater on the line bisecting the lunar disc) <26> which can be made plausible only by the most skilful selection of modern lunar photographs <27>. And Galileo's struggle for the Copernican system can be considered 'scientific' only because he happened to be right, otherwise he broke every rule of the game.

Now, this is the sort of thing that can easily 'blow the mind' of a student for whom (like so many) the authority of science is as absolute as theology ever was in the Middle Ages. After such an experience of shock and disillusion, the student may be ready to awaken to the truth that there is no truth to awaken to. In his role of awakener, Feyerabend may be considered as a Zen master. But the analogy is very imperfect: a traditional Zen master operated in an 'I-thou' relation, so that the searcher would be genuinely enlightened and not destroyed. Providing an anonymous reading public with an exhibition of a lot of sacred images being sprayed by a philosophical machine-gun is a very different activity indeed.



For this reason, and another as well, Feyerabend may come under suspicion of being in effect (though certainly not in intention) a Fascist <28>. For what he offers, to replace the old ideal of philosophy of science, is confused and unconstructive. It is along the lines of allowing everyone to 'do his own thing' freed from the constraints of convention or of social or logical propriety. Those who recall the connections of Nazi 'German-Folk' ideology and religion with earlier currents of 'Romantic' and antimechanical philosophies are justifiably troubled by Feyerabend even more than by other 'counter-culture' prophets. Feyerabend's prescriptions may be all very well after the anarchist Utopia has been achieved; but in the short run it may mean destroying the intellectual barriers to the victory of arbitrary will and brute force in intellectual, and hence social, matters.

Feyerabend could reply to such an accusation with the rejoinder that for most of the world's peoples, aside from the white, mainly-male middle class beneficiaries of high culture, this is precisely the unspeakable state of affairs already; and that it is both concealed and sanctioned by our dominant ideas of Science and Method. A chronicle of the oppression and mutilation of subject peoples (including the whole female sex), at the hands of scientific medicine, would go far to establish the plausibility of his case <29>. Indeed, the only really conclusive answer to his critique is the classic of a departing reactionary, 'Après moi, le déluge - so that all but the most fanatical radical revolutionaries realise too late the benefits of a rule of law that had been at least consistent, however harsh and unjust.

So Feyerabend is best seen as a searcher, having tried one self-advertised guru after another, and every time finding feet of clay; and finally coming to rest somewhere in a romantic-radical counter-science milieu. His politics is that of a cranky fringe, but his philosophical armoury is still strong enough to bring pain and respect, if not always appreciation and agreement, from his conventional colleagues. In him, the ideological motivation for philosophy of science has become fully explicit, indeed dominant, and finally quite destructive of the tradition from which it stemmed.



If that tradition had been truly 'positive' like the science it proclaimed, and had tough and resilient roots in a real understanding of its practice, it would not have been so vulnerable to the assault of its critics. But, as I have shown here, the image of 'science' that was invoked in that programme was itself the product of an ideology, though sometimes unselfconsciously applied; hence when the ideology lost plausibility, the technical articulations made by philosophers of science were discovered to be hollow and brittle. Two thinkers, one a quite unsubtle conservative, and the other an eccentric radical, were sufficient to destroy the foundations of that whole intellectual edifice.

#### Where do we go from here?

I do <u>not</u> wish to say that any philosophical system is only a tissue of rationalisations or assumptions that enjoy a temporary plausibility. Although philosophy is very different in degree from the more 'positive' sciences that enjoy a more direct foundation in controlled experience, it too leaves behind a residue of achievement, in understanding rather more than in detailed knowledge, as each great movement or school passes through its cycle of growth and decay. But when all the signs point to a philosophical cycle nearing its end, it is time to see whether the world which was its passionate concern still exists.

The ideology of the previous phase of philosophy of science was derived from a centuries-long battle with 'religion'. This lay not so much in the realm of individual faith, as that of pretensions to exclusive knowledge, and of claims to political power partly on that basis. Now, in the later twentieth century, that old battle is over; the Christian Churches are in an excited and turbulent state that may indeed herald a great re-birth but which certainly does not promise either the renewed obedience of the faithful or deference of the secular powers. Instead, some at least of the clerical evils that motivated the endeavours of Enlightenment have now been inherited by the apparatus of antireligious State power. And from science itself have come new evils, inconceivable once magic was discredited until the advent of the atomic bomb. So that those who still try to identify science with the humane, civilised values now find themselves in a confused night battle, where friend and foe are ever more difficult to distinguish.

The old epistemological problems of science are, therefore, no longer fruitful for our understanding of that great creation of the human intellect. As they have become isolated from their roots in committed experience, they can provide no effective defence against the suicidal application of reason in Feyerabend's arguments. I suggest that they be given a rest, and that new critical insights be applied to the analysis of science, not in a spirit of angry demystification, but as a complement to progress already being made in the history and the sociology of science. There, studies of the actual conditions and constraints on scientific work are providing a picture that is rapidly being enriched, of how science can have both successes and failures, virtues and vices, without being the subject of one simplistic verdict on the degree of its adherence to the Good and the True.

I can imagine two sorts of changes in the philosophy of science: in its style, and in its topics of inquiry. Concerning the former, we can recall that the Viennese endeavour had a very Middle-European flavour. In its struggle against what it claimad were the existing politically-motivated dogmas of theology and metaphysics, it advanced its own dogmatic and simplistic version of Truth through Science. In this respect it participated in a stylistic tradition going back to Descartes (with his deductions of all truths from God's essence), and Galileo (proclaiming 'the conclusions of natural science are true and necessary'). The harshness, indeed arrogance, of their doctrines in natural philosophy was related to that of the expression of the mathematical sciences which were their exemplars. This survives still, most noticeably in teaching, but also in popularisation. Nowhere do the assertions of such sciences (as traditionally expressed) make a place for criticism; as Popper has observed, the uncritical attitude fostered by teaching in the 'normal science' mode is a danger to science and to civilisation <30>.

If, as I believe, the main problems in comprehending science now derive from the effects of its great successes and strengths rather than from external attacks, a different style of philosophising may be appropriate for the next phase. I would rather not try to give it a single label; I might describe it in terms of some polar oppositions. It would be empirical rather than rationalistic; motivated towards improving practice in known difficult areas rather than articulating standards of unattainable perfection; and progressing by the sharing of insights, informally expressed, rather than by the production of abstract formal systems. The best example of this that can be shown is that of Francis Bacon, whose goal was a collection of 'aphorisms', analogous to those which in his opinion made English pastoral theology so much superior to its dogmatic continental counterparts. Those philosophical writings cast as genuine dialogues can also serve as examples; though it seems that very few philosophers have been able to resist the temptation of having the correct line triumph in all their intended debates. I am well aware that style is not a panacea; and those raised in one style might have grave disappointments in trying to transform their thoughts to another. Here I am expressing my personal reaction to the ultimate inadequacies of the Viennese style: if it proved impossible to articulate a single coherent criterion for distinguishing the true-and-good from its vicious rivals and imitators, perhaps the attempt should, in retrospect, be seen as a personal endeavour rather than as a scientific exercise. And with changed times and problems, we should feel free to think again about style and about the deeper criteria of genuineness of cognitive discourse which it reflects.

#### Footnotes

- 1
- See, for an example of a recent textbook, A. Chalmers, <u>What Is This Thing Called</u> <u>Science?</u> (Open University Press, 1978). See, for instance, the remarks in I. Lakatos, 'History of Science and its Rational Re-constructions', in <u>The Methodology of Scientific Research Programmes</u> (Cambridge, 1979) a. 127. 2 1978), p. 137
- D. Harvey, <u>Explanation in Geography</u> (London, Arnold, 1969) is a good example of such an attempt; the author subsequently turned to politically radical interpretations of urban geography
- Vienna Circle, The Scientific Conception of the World (1929) (pamphlet) (Reidel, 1973) also in M. Neurath and R.S. Cohen (eds.), <u>Otto Neurath: Empiricism and Sociology</u> (Reidel, 1973), emphasis in original. 4

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- (Reidel, 1973), emphasis in original.
  A. Naess, The Pluralist and Possibilist Aspect of the Scientific Enterprise (London, Allen & Unwin, 1972), p. 135.
  I return to the question of 'style' towards the end of this essay.
  E. Mach, The Science of Mechanics (1883, many translations and editions).
  See 'Science: Conjectures and Refutations', in <u>Conjectures and Refutations, The Growth of Scientific Knowledge</u> (London, Routledge, 1963@, p. 36.
  See K. Popper, <u>Unended Quest</u> (London, Routledge, 1963@, p. 36.
  See K. Popper, <u>Unended Quest</u> (London, Fontana, 1976), pp. 36-37.
  K. R. Popper, 'Back to the Presocratics' in <u>Conjectures and Refutations</u> is a very attractive attempt to show how Popperian method and ethics were responsible for 'the Greek miracle' in natural philosophy.
  G. Holton, 'Mach, Einstein, and the Search for Reality', in <u>Thematic Origins of Modern Thought</u> (Harvard, 1973), p. 236.
  K. Popper, The Logic of Scientific Discovery (London, Hutchinson, 1959).
  T.S. Kuhn, <u>The Structure of Scientific Revolutions</u> (Chicago, 1962), p. 170. 11
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- 14
- 15 Ibid., p. 5. Ibid., pp. 5-6.
- K. Popper, 'Normal Science and its Dangers' in I. Lakatos and A. Musgrave (eds.), <u>Criticism and the Growth of Knowledge</u> (Cambridge, 1970).
   P. Feyerabend, 'Consolations for the Specialist' in Lakatos and Musgrave, <u>op. cit.</u>, pp. 17 18
- 200-201. I. S. Kuhn, 'Reflections on my Critics' in Lakatos and Musgrave, op. cit., p. 245.
   I. Lakatos, 'Introduction. Science and Pseudoscience', in <u>Methodology of Scientific</u>
- Research Programmes (op. cit.). I. Lakatos, Proofs and Refutations. The Logic of Mathematical Discovery (Cambridge, 21 I. Lak 1976).
- G. Polya, <u>Mathematics and Plausible Reasoning</u> (2 vols.), (Oxford and Princeton, 1954). Lakatos and Musgrave, <u>op. cit.</u> This work includes a much revised version of Lakatos's 22 responses.
- Ibid., p. 151.
- Bild, p. 191. B. Feyerabend, <u>Against Method</u> (London, New Left Books, 1975). Galileo, <u>The Starry Messenger</u> (1610), second drawing of the moon, with crater com-pared to Bohemia; see S. Drake, <u>The Discoveries and Opinions of Galileo</u> (New York, Doubleday Anchor, 1957), p. 35. S. Drake, <u>Galileo at Work, His Scientific Biography</u> (University of Chicago Press, 1978), p. 145. 26
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- S. Drake, <u>Galileo at Work, His Scientific Diography</u> (University, C. S. Drake, <u>Galileo at Work, His Scientific Diography</u> (University, C. S. Berlier, 1978), p. 145.
  See E. Gellner, Beyond Truth and Falsehood', <u>British Journal for the Philosophy of Science 26</u> (1975), pp. 331-43. Feyerabend's reply, 'Logic, Literacy and Professor Gellner', <u>ibid.</u> 27 (1976), pp. 382-91, where he denies being a Fascist by claiming court-jester and Zen-master status. The reply is reprinted with modifications in <u>Science in a Free Society</u> (London, New Left Books, 1978), pp. 145-53.
  P. Feyerabend, <u>Science in a Free Society</u>, p. 175. For partial confirmation of one of his accusations see <u>Science vol. 204</u> (22 June 1979), pp. 284-5: There is no scientific evidence that a radical mastectomy gives any better results than a modified one for early breast cancers, according to the consensus meeting held on 5 June at the National Institute of Health'. Interesting studies of the role of 'Medical Science' in the oppression of women are B. Ehrenreich and D. English, <u>For Her Own Good</u> (London, Pluto Press, 1979) and G.J. Barker-Benfield, <u>The Horrors of the Half-Known</u> Life (NY, Harper, 1976). 29 (London, Pluto Press, 1979) and G.J. Barker-Benfield, <u>The Horrors of the Hal</u> Life (NY, Harper, 1976).
   K. Popper, 'Normal Science and Its Dangers' in Lakatos and Musgrave, <u>op. cit.</u>

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## Comment on Ravetz **Roy Edgley**

Ravetz argues that the Viennese tradition in the philosophy of science developed an idealised and unrealistic conception of science not because it was doing pure philosophy, specifically epistemology, but, on the contrary, because of its political commitments. Participating in a trend going back to Enlightenment rationalism, it shaped an understanding of science by contrast and in conflict with the 'dogma and superstition', the metaphysics, embodied in unprogressive social institutions and movements. As Ravetz says, the ideology of this tradition in the philosophy of science was 'derived from a centuries-long battle with "religion"'. However, 'that old battle' is now over, and anyway 'from Science itself have come new evils': so it's no longer necessary or possible for science to have this ideological value and function. Instead, there should be an end of ideology in our understanding of science. In a suggestion deliberately modest, even deflating, by comparison with the triumphalist rationalism of the tradition now spent, he proposes changes in style and topic, to allow a more 'positive' study of the reality of science in its socio-historical context, with more openness in discussion rather than 'a spirit of angry demystification', recognition that 'science can have both successes and failures, virtues and vices', and the aim of 'improving practice in known difficult areas'.

Ravetz must be right in advocating a more realistic study of science, as it is and has been and could be, in relation to its socio-historical context. But does his argument that science is no longer ideologically sensitive focus too exclusively on natural science and its philosophy, in the Enlightenment and the 20th century, ignoring the 19th century and the growth and struggles of social science? The old battle between science and religion may be over, but, as he points out, the Viennese tradition used the name and nature of science to attack not religion but political ideologies. In its concentration on natural science, that may have been the tail-end of the Enlightenment battle. But reviving even that feature of the Enlightenment would hardly have been necessary if the 19th century had not intervened, with its Romantic reaction against the Age of Reason, its continuing secularisation of society, and the transition from social philosophy to social science. In changing the target of science and its philosophy from religion to secular politics, the Viennese tradition was indebted to the 19th century and was taking part in a new battle: between science as social science and the non-scientific social ideas involved in political systems, institutions and movements. In philosophy, the relevant focus has shifted from the philosophy of (natural) science to the philosophy of social science.

It must be agreed too that 'from Science itself have come new evils', and that 'philosophy' in this area inevitably tends to generate abstract Platonic ideas of perfect science that are of little use in providing criteria for criticism either of other ideas or of actual scientific practice. But Ravetz acknowledges the possibility of and need for improving scientific practice, and the criteria involved here must be applicable to ideas produced by non-scientific practices. What is distinctive of his position is its advocacy of a 'positive' piecemeal approach and its hostility to what he calls 'a spirit of angry demystification'. But there is a third way between the high priori road of philosophy and the piecemeal positive approach, namely the approach of theoretical science itself. And however much the Viennese tradition was misled by its political commitments, it does not follow that political commitment as such, and any 'angry demystification' that goes with it, necessarily obscures and misleads. A political position provides a point of view from which important matters otherwise obscured may become visible. Indeed, is Ravetz's own position nonpolitical, or does his 'end of ideology' theme operate as the familiar disguise for the liberal position? Does he assume that though the Viennese tradition has come to an end, the socio-political commitment of Popper and Lakatos is still viable and to be preferred? Is it his view, or just attributed to Lakatos, that the enemies of reason 'could come equally well from the "Left" as from the old Right' - not, apparently, from the centre?

It may be, of course, that Ravetz intends his epithet 'angry demystification' to apply to Popper and Lakatos also. Liberalism can afford to be complacent when liberalism is the status quo and not under serious threat, when 'anti-dogmatism' standardly functions as a justification for inaction or for the 'moderate' action that leaves everything (basically) as it is. But Popper and Lakatos lived in societies where neither condition held, and both, but Lakatos in particular, were walking embodiments of liberal paradox. The fact is that liberalism and its epistemological counterparts, scepticism and anti-dogmatism, are positions that compete for acceptance with other positions conflicting with them, and they themselves can be held, taught, and practised either liberally, sceptically, and anti-dogmatically, or the reverse. Popper made a grotesquely unsuccessful attempt on these problems in his paper 'On Violence', and his own educational practice was made the butt of a cruel joke suggesting that the title of his best-known book should have been 'The Open Society By One Of Its Enemies'. As for Lakatos, the conjecture Ravetz refers to, recalling Koestler's Darkness at Noon, should be compared with the episode in Orwell's 1984 in which Winston Smith undergoes an ordeal similar to Koestler's Rubashov. Ravetz's conjecture is that Lakatos's anti-dogmatism in mathematics might provide the support for a survival strategy in such an ordeal. Orwell on the other hand depicts Winston Smith's resistance as being broken down precisely by the process by which his confidence in simple mathematical truths is undermined. The implication of Orwell's idea is that ultimately and in some circumstances the survival of liberalism requires unshakeable conviction. Shall we say dogmatism? Lakatos's anti-dogmatism, apparently, was sufficiently dogmatic for him to be convinced of a proposition even less certain than those of mathematics, namely 'that his version of Popperian liberal philosophy of science was central to the defence of civilisation'. He was also convinced that defending liberal civilisation required not only Popperian philosophy but also what he used to refer to as 'the American nuclear umbrella'; which reminds us that our Western liberal democracy, whose essence, according to Ravetz, is 'open dialogue', is dogmatic to the point of arming itself to the teeth against alternatives. Lakatos's opposition to the Left in the late 1960s and early '70s was a kind that in my hearing drew from a prominent non-Marxist academic a memorable question: had he changed from a Stalinist witch-hunter to a liberal witch-hunter? Was he an anti-dogmatist or an officer of the thought-police? Or does one require the other?

The politics of Ravetz's proposals perhaps come out in another way. I've already suggested that Ravetz's argument, like the Viennese tradition itself, to say nothing of the 20th century English analytical movement, ignores the 19th century. Yet on several crucial issues there is substantial agreement between him and one strand of the 19th century reaction to the Enlightenment, the strand represented by Hegel and Marx. Ravetz tells us that 'The old epistemological problems of science are ... no longer fruitful for our understanding of that great creation of the human intellect'. Hegel and Marx saw that long ago, but were ignored or misunderstood by both the Viennese and English philosophical movements of the 20th century. Ravetz suggests that one of the most important things we have recently been discovering about 'those old epistemological problems of science' is that they tended to ignore both the practical aspects and context of science and its historical development. Hegel and Marx would have agreed, and might have asked why it has taken us so long to rediscover these things. Ravetz asserts that '... from Science itself have come new evils, inconceivable once magic was

discredited until the advent of the atomic bomb...'. Marx, though he firmly rejected the Romantic reaction against science, didn't need to wait till the atomic bomb to recognise the evils that came from science, in its form in the technology of capitalist manufacture. However, though Marx agrees with Ravetz in rejecting purely philosophical accounts of science, he insists on the need for, and begins to provide, a more theoretical approach to the understanding of science's 'virtues and vices', its political relations, and its potentialities for piecemeal improvement in a spirit eschewing 'angry demystification'. As far as I can see, nothing in Ravetz's paper rules that out, though his underwhelming proposals take no account of the possibility.

### NEWS REPORT Socialist Society

The Socialist Society AGM was held on 24 March at County Hall, London.

Although speakers addressed problems of the present political conjuncture, this was essentially a meeting to discuss the future role of the Society. The wider discussion generally functioned as a way into this question.

The 'future role' debate was, in fact, critical, noted Ralph Miliband. The meeting signalled a low point in the fortunes of the Society, a point of 'renewal' or 'withering away'. Perhaps this was somehow a microcosm of the state of disarray on the Left. At the same time, the Tories had mounted a sustained and successful attack on democratic rights, under a 'constitutional veneer'. Authoritarianism was convincingly presented as the defence of public rights and order. Miliband suggested that the Society could have a role in forming local committees for the defence of democracy.

John Palmer argued that the dominance of the Right remained 'fragile'; working class resistance had not been broken <u>fundamentally</u>. He saw a need to reassert the utopian and visionary dimension of the argument for socialism within the labour movement.

What had happened to feminism within the present conjuncture, suggested Elizabeth Wilson, could best be described as a process of fragmentation accompanied by growing sectarianism. One manifestation of this sectarianism was the tendency to elevate the <u>sexual</u> division above that of <u>class</u>. Many feminists were in fact anti-union, despite the fact that women are well represented (numerically) in unions and are active in strikes. Women were not outside class politics, and a recognition of the way class divisions fracture feminism itself was required.

A number of proposals were made about future activity. The Society could: function as a catalyst in the struggle to defend democratic rghts, via university-based discussion groups (John Palmer); provide an alternative programme for the Labour Party (Mike Rustin); operate as a unit of socialist research and education (Hilary Wainwright); serve as a forum for trade union activists (Robin Blackburn).

Organisationally, three types of proposal seemed to be coming from members, suggested Sarah Benton; (a) the research/education function and style of grouping; (b) an emphasis on local groups; (c) centrally co-ordinated local discussion groups.

It was decided to establish a working party to look at the various suggestions on a way forward for the Society.

In the morning session, Ken Livingstone had spoken on the problem of overcoming bureaucratic inertia, faced by those attempting to implement socialist policies at the GLC.

Perhaps the Socialist Society would find a role in providing an 'alternative bureaucracy' to the mandarins of County Hall!

Howard Feather