Matteo Motterlini (editor)
For and Against Method
University of Chicago Press

To my parents

Were I to choose an auspicious image for the new millennium, I would choose that one: the sudden agile leap of the philosopher who raises himself above the weight of the world, showing that with all his gravity he has the secret of lightness.

Italo Calvino

Preface

This is how Paul Feyerabend remembers Imre Lakatos in his autobiography, *Killing Time*:

I lectured at [...] at the London School of Economics [...]. "Science has many holes", I said in passing. "A Popperian triviality", shouted Imre Lakatos. That shut me up; but I soon smiled at the incident. [...] The lecture hall at the London School of Economics was directly opposite Imre's office window. In spring and summer, when the windows were open, Imre could hear every word I said. Feeling outraged, or simulating outrage at the drift of my story - with Imre you were never sure - he left the Great Thinkers who happened to be with him, came over, and tried to set things right. Imre and I [...] differed in outlook, character, and ambition; yet we became really good friends.

The stronger their friendship became, the more challenging their intellectual conflict. The work which was to determine Feyerabend's fame and notoriety, *Against Method*, stemmed from Imre's challenge to Paul: "In 1970 Imre cornered me at a party. 'Paul', he said, 'you have such strange ideas. Why don't you write them down? I shall write a reply, we publish the whole thing and I promise you - we shall have a lot of fun.' I liked the suggestion and started working."

Lakatos died suddenly of a heart attack on 2 February 1974 without having written his reply; yet the reader has here the chance to reconstruct Lakatos's original counter-arguments and, at the same time, enjoy the "fun" the two authors had in matching their views and seeing each other as rivals.

This volume shows Lakatos and Feyerabend's intellectual relationship through a selection of hitherto unpublished writings from the "Archive of Professor Imre Lakatos of the British Library of Political and Economic Science". The first of these is the transcript of a series of *Lectures on Scientific Method* held by Lakatos at the London School of Economics in the period from January to March 1973, thus giving in effect his final view on the subject. Next come Feyerabend's *Theses on Anarchism* (1973) which contain a sketch of Feyerabend's attack to the rational position he would later expound in his *Against Method*. The latter being - in Feyerabend's words - "a long and rather personal *letter* to Imre and every wicked phrase it contains was written in anticipation of an even more wicked reply from the recipient." The *Correspondence* Feyerabend and Lakatos exchanged between December 1967 and February 1974 clearly shows that Feyerabend's remark is far from being merely rhetorical, for each of his views is here constantly contested and debated by Lakatos up to his very last days. The whole material is thus of considerable intellectual importance to anyone concerned with the development of the philosophical views of Lakatos and Feyerabend, or indeed with the philosophy of science in general.

For those unfamiliar with the area, the *Introduction* to this volume - in the format of a imaginary dialogue between Lakatos and Feyerabend - aims to give the main themes of the debate. Lakatos's *Lectures,* in turn, provide a fascinating insight into the problems of philosophy of science and their relevance for pedagogical and socio-political matters - one that remains accessible to the general reader; while Feyerabend's *Theses* challenge Lakatos's "rationalistic cliff-hanger" from an anarchist stand. Finally, the *letters* included here reveal how much the two friends relished the flavour of philosophical controversy, regarding it as the antidote to that conformism so widely present in the world of academics, educationalists, specialists and professional politicians.

Acknowledgments

This book would have not appeared were it not for Grazia Borrini Feyerabend, Gillian Page, John Watkins and John Worrall: to them and to the Imre Lakatos Memorial Fund I should like to express my special gratitude. I am grateful to Alex Bellamy, Nancy Cartwright, Marco Del Seta, Donald Gillies, Giulio Giorello, Colin Howson, Ladislav Kvasz, Brendan Larvor, Spiro Latsis, Marco Mondadori, John Preston, Elio Sindoni, Mauricio Suárez, Peter Urbach, Thomas Uebel and Elie Zahar for many useful suggestions. I should also like to express my thanks to Susan Abrams for unfailing help and encouragement. I am indebted to Francesco Guala, with whom I discussed most of the problems raised in the editing process, and to Benedetta Tiana for stylistic improvements. The Centre for the Philosophy in the Natural and Social Sciences and the University of Ferrara are also gratefully acknowledged. Any mistakes fall of course under my own responsibility.
M.M.
London, January 1998

Note. Information regarding the history of the material published in *this volume* and the editing process it has undergone appears in the introductory note to each item. All references in *this volume* are in the *Bibliography* at the end. Editorial footnotes are marked by Arabic numerals.

Contents

Introduction

A Dialogue

Matteo Motterlini

> A dialogue is a discourse consisting of question and answer on some philosophical or political subject, with due regard to the characters of the persons introduced and the choice of diction. The dialectic is the art of discourse by which we either refute or establish some proposition by means of question and answer on the part of the interlocutors.
>
> Diogenes Laertius

The following dialogue between Lakatos and Feyerabend is obviously nothing more than fiction; but over the years a real dialogue took place between the two friends. This consisted in a genuine, lengthy, continuous and outspoken exchange of letters and papers which shows the two men taking stands in the discussion for *and* against *method. My fictitious reconstruction mirrors their own contributions, but paraphrases them for stylistic reasons. I refer to the original texts in the footnotes.*
The rhetorical form of the dialogue is well captured in the above fragment by Diogenes Laertius. The reason for adopting it here is given by the two imaginary interlocutors explicitly at the beginning of their discourse.

Paul Feyerabend: Rumour has it, dear *Imre*, that while you can freely discuss ideas in a loose way, in letters, phone calls and at dinner, academics will always prefer an essay or a book. And any paper of this kind has a beginning, a middle and an end. There is an exposition, a development and a result. After that the idea is as clear and well-defined as a dead butterfly in a collector's box.[1]

Imre Lakatos: Plato thought that the *gulf* between *ideas* and *life* could be bridged by *dialogue* - not by a written dialogue which he considered but a superficial account of past events -, but by a real, spoken exchange between people of different backgrounds. I agree that a *dialogue* reveals more than an essay. It can show the effect of arguments on outsiders. It makes explicit the loose ends which an essay tries to conceal by showing the *inconclusiveness of 'conclusions'*[2] ...

Paul: ... and, above all, it can demonstrate the chimaerical nature of what we believe to be the most solid parts of our lives. And with this, we have already reached our topic. In particular, I would like to discuss the tremendous *gulf* that exists between the various *images* of science and the *'real thing'*. I cannot but think that the fine dividing line between scientific truth and epistemological 'castles in the air' is in fact very similar to the line we draw between 'normal' and 'insane' people: a trait which recurs among the latter is the tendency to detach themselves further and further from reality.[3]

Imre: I agree that any attempt to reform science by bringing it closer to the abstract image philosophers have of it is bound to damage and may even destroy science.[4]

Paul: The point on which we disagree, though, is your attempt to save both 'Progress' and 'Reason'. You claim there are standards which are both so flexible that they leave science leeway in which to *progress* and at the same time substantial enough to let *reason* survive.[5]

Imre: As a matter of fact, my standards apply to series of theories (research programmes) and not to individual theories; they judge the evolution of a programme over a period of time, and not its shape in a given instant; they compare its growth with that of rival programs. These criteria are therefore open both to the history and to the practice of science. Moreover, I term 'progressive' any programme which predicts events confirmed by subsequent research, thereby leading to the discovery of 'new' facts. I term 'degenerating' any

programme which makes no such prediction, but simply 'saves' data discovered by its rival. Since I do not believe there exists any 'natural saturation point' in a programme, I can also distinguish between *falsification* and *rejection*, something Popper could not do. In this way I am entitled to 'shift' his initial problem - the demarcation between science and pseudoscience - to the the new one of demarcating between *good* science and *bad* science (i.e. between progressive and degenerating programmes).[6]

Paul: I'm with you, but one question keeps bothering me, and that is whether there is any *pragmatic* implication in evaluating theories with your kind of standards.

Imre: Methodological standards act like teachers: they give marks to theories. Moral criteria used in judging individuals have grave practical implications in education; similarly, scientific criteria used in judging theories have deep consequences for scientific method.[7]

Paul: Are you saying that if a research programme is judged better than a rival one, scientists *ought* to work on the allegedly superior one?

Imre: I am actually injecting some Popperian elements into the judgment of whether a programme progresses or degenerates, or whether it is overtaking another one. I am giving you criteria for progress and stagnation within a programme, and rules for the 'elimination' of entire programmes. Should a programme explain in a progressive way more than a rival programme accounts for, then it 'supersedes' the latter, and the rival one may be 'rejected' or simply 'shelved'. You cannot at this point fail to understand what the *pragmatic* meaning of 'rejecting' a programme is: very simply, it means the decision to *cease working on it*.[8]

Paul: OK, but it is easy to see that standards of your kind have practical force only if combined with some *time limit* after which to keep working on a degenerating programme would be '*irrational*'. If you accept the idea of the time limit, then unfortunately arguments very similar to the ones you used against naive falsificationism backfire against your own standards. Consider that if it is unwise to reject faulty theories the moment they are born because they might grow and improve, then it is also unwise to reject research programmes on a downward trend because they might recover and attain unforseen splendour: a butterfly emerges when the caterpillar has reached its lowest state of degeneration.[9]

Imre: Don't get me wrong here. My methodology deals exclusively with fully-fledged research programmes, but has no intention of handing out *advice* to the scientist on how to arrive at good theories or on which of two rival theories he should work on. The standards of appraisal I put forward explain why it is *rational* to accept Einstein's theory rather than Newton's, but they do not force the scientist to work on the Einsteinian programme rather than the Newtonian one. I can only judge what scientists have done: I can say whether they have progressed or not. But *I cannot give them any advice - nor do I wish to.*[10]

Paul: And yet, at the beginning, the bold project of "The *logic* of scientific discovery" was aimed at describing those rules which govern the *acceptance* and *rejection* of scientific theories. Rules that should have functioned as a *code of intellectual honesty* whose violation was intolerable.[11] What then is the point of laying down rules which may indifferently be either followed or ignored? You're like the author of a cookbook who describes the recipe for making *good* pizza and then remarks: "Of course, I am not telling you what to do, but whatever you do, keep a record of it".[12] Your *standards* are only *verbal ornaments*: a remembrance of past happier times when it was still thought possible to run a complex and often catastrophic business like science by following a few simple and 'rational' rules. As a matter of fact, your flexible scientific 'method' is nothing but a disguised version of my *anything goes*.[13]

Imre: There is freedom ('anarchy', if you like) in choosing which programme to work on, but the products *must be judged*. You are conflating *methodological appraisal* of a research programme with *heuristic advice* on what to do. One may rationally stick to a degenerating programme until it is overtaken by another, and even after. What one must not do is ignore its poor record. Playing a risky game is perfectly rational (and honest) : what is irrational (and dishonest) is to pretend the risk isn't there, or to belittle it. Everyone is free to follow his own peculiar inclinations, but only as long as he publicly admits the state of open competitiveness.[14]

Paul: I still think you are not clear enough in your distinction of *rationality* and *honesty*: a person can easily be rational and dishonest (or irrational and honest). Dillinger was surely dishonest, but it would be hard to show that he was irrational with regard to his research programme, which just happened to be organised crime.[15] If your only piece of *advice* is to be *honest* in judging the evidential pros and cons of the various research programmes, then consider how *futile* the point of view which allows a thief to steal as much as he wants, and yet be praised by the police and by everybody else as an honest man provided he admits to stealing. If your methodology differs from anarchism in this sense *only*, then I'm ready to become one of its fans. Who would prefer criticism to praise, if all he has to do is describe his actions in the language of a particular school?[16]

Imre: Wait a second. I am not saying that people who support a degenerating research programme should enjoy as much freedom as you seem to imply. In fact, they should *not* however be allowed to publish their papers which contain, in general, solemn iterations of their positions, or attempts to reabsorb counter evidence by *ad hoc* adjustments. Editors *should* refuse publication, research foundations *should* refuse them funds.[17]

Paul: And here we come across yet another "strange case of Dr Jekyll and Mr Hyde". *First* you give full rein to your *anarchic* vein, maintaining that the only irrational kind of behaviour consists in denying the state of the programme one is working on. *Then* you entreat publishers and societies to refuse printing and funds! Let me also add that I was not at all confusing 'methodological appraisal' with 'heuristic advice', as you seem to suggest. I was rather insisting that there is a legitimate link between them. You are the first to admit this when, betraying your *authoritarian* nature, you strengthen your standards *not* on an argumentative level, but by shaping a historical and social situation which renders it difficult, *in practice*, to cultivate a degenerating programme. Taken by themselves your standards are incapable of ruling out the most outrageous behaviour; taken in conjunction with a certain kind of conservatism, on the other hand, they have a subtle but firm influence on the scientific community. You want it both ways: you're making the omelette (you have more liberal standards), keeping the eggs (you have them used in a conservative way) and even passing as a rationalist! [18]

Imre: I'm not 'passing' as a rationalist, I'm a full blooded rationalist!

Paul: You abhor irrationality, that's true. And yet you can exclude it only by adopting measures which turn out to be irrational when set against your very own standards! This of course doesn't make you a willing anarchist, but it makes you a rationalist who by misadventure ends up in irrationality.[19]

Imre: But still there is a considerable difference between us.

Paul: There is a considerable difference in *rhetorics*. Combining common sense standards of scientists with the methodology of scientific research programmes, you utilize the intuitive plausibility of the former to support the latter: a splendid Trojan horse that can be used to smuggle real, honest (a word you hold so dear) anarchism into the minds of our most dedicated rationalists. You are much better

at this than I am, since rationalists are constitutionally incapable of accepting anarchism when it is offered to them undisguised. One day, of course, they will realise that this is what happened. That will be the day they will finally be ready for anarchism, pure and simple.[20] I have to admit that your plan is diabolical. But remember, my name is *Lucifer*, so it is *I* who bring the *light*, *not* you![21]

Imre: Yes, but Lucifer denotes the chap who brings *false* light, while I am shrouding them *in the darkness of truth*.[22] My methodology is a theory for characterising real cases of growth of knowledge and distinguishing them from impostures. Its appraisals are retrospective: they only tell us that a programme has been better than its rival *up till now*, without in any way deciding anything for the future.

Paul: This, however, means that any piece of advice based on past performances will be totally *arbitrary,* and we are back at the start.[23]

Imre: No, we aren't. If the methodology of research programmes aims to be something more than a descriptive account of the past performance of theories, then it must provide its methodological rules with an extramethodological support of a conjectural kind. I once asked that Sir Karl Popper admit a 'whiff' of inductivism in order to relate the scientific gambit of pragmatic acceptances and rejections to verisimilitude.[24]

Paul: A 'whiff'? I would rather say a full-blown storm.[25]

Imre: Call it what you wish. The point is that only a similar 'inductive principle' can turn science from a mere game into an epistemologically rational activity; from a set of lighthearted sceptical gambits pursued for intellectual fun into a serious fallibilistic venture of approximating the 'Truth of the Universe'.[26]

Paul: But what have we gained?

Imre: I can now give a *positive* answer to your previous question concerning the value of any *practical indications* based on judgements which refer exclusively to scientists' *past* performances. Thanks to our '*conjectural* principle of induction' the fact that our appraisals may in the future be contradicted does not constitute a good reason for not relying on them now. Even though the future is unpredictable, programmes chosen at random are not all equally promising. Thus, from an appraisal such as: "programme A has been degenerating up till now whereas programme B has been progressing", one may possibly derive a *piece of practical advice* such as that the scientific community should devote most of its intellectual and economic resources to programme B (and note that *most* is not equivalent to *all*!). This solution cerainly offers 'all the advantages of honest theft over dishonest toil'; but it might be that in this area 'honest theft' is our only option.[27]

Paul: So what's left of the anti-inductivist bequest of Popper who is commonly known as the slayer of Logical Positivism and as the one who solved (in a negative way) 'Hume's problem'?[28]

Imre: It seems to me that Popper has to admit that methodological appraisals are interesting primarily because of a *hidden* inductive assumption. I.e. that if we act in conformity with these appraisals, we have a better chance to get nearer to the Truth than otherwise.[29] This reminds me of Columbus when the "sea current carries exotic plants, animal carcases, finely carved wooden objects, and he visualizes the far-off and yet unknown land from which these objects come".[30]

Paul: One of the examples Ernst Mach loved making when he wanted to show the vital importance of conjectures, even of the most speculative ones.[31]

Imre: Neither can we do without bold hypotheses in the theory of knowledge. The fact that one particular assumption is put forward as pure speculation shows that we are conscious both of its lack of proof and of its necessity. There is nothing

wrong with *fallible* and *speculative* metaphysics, but only with interpreting such metaphysical statements as infallible inductive principles.[32]

Paul: I like your candid fallibilism, which is surely a step in the right direction. I mean towards releasing our most deeply rooted beliefs from their putrid foundations. Yet the task of scientists no longer lies in "searching for the truth' or 'improving predictions', but rather, in the words of Sophists, "in making the weaker case the stronger one, thereby to sustain the motion of the whole."[33]

Imre: So, from your point of view, as I understood it, it is not 'the *truth* [that] will make you free'?[34]

Paul: *The truth*, whatever it is, *be damned*. Play, fun and fiction will make you free. Someone who laughs looks intelligent (much more so than someone who explains her 'profound convictions'). She seems magically lifted out of the sea of fear, poverty and egoism in which fate threw her and in which she is kept by the 'truth'. What we need is to take things *lightly*.[35] Were I to choose an auspicious image for the new millennium, I would choose this one: the sudden agile leap of the philosopher who raises himself above the weight of the world, showing that with all his gravity he has the secret of lightness.[36] (Of course, I would be talking about the lightness of thoughtfulness rather than the lightness of frivolity. In fact, the thoughtful kind of lightness can make frivolity seem dull and heavy.) You have a gift for laughter, even where your own position is concerned; thus for me you are a good guy.[37]

Imre: But we live in a world moulded by science: isn't this reason enough to study science?

Paul: Of course it is. But when there is an invasion of locusts, people study locusts *in order to be free of them* not so as to turn them into new found gods![38]

Imre: So here's my idea of pesticide. I have claimed that the unit of appraisal for the growth of knowledge is a series of theories, in which each one is obtained by adding some auxiliary clauses in order to accomodate certain anomalies and produce new predictions. But we must also require that, at least once in a while, the increase in content should be corroborated: the programme as a whole should also display an *intermittently* progressive empirical shift.[39]

Paul: And this is where your recipe fails to work: how should we conclude that the research programme in question has 'run out of steam' and should therefore be abandoned?In fact, what appears to be a sequence of degenerating adjustments may just happen to be the initial phase of a long progressive development. After Aristotle and Ptolemy, the idea of the Earth moving - that weird, ancient and 'entirely ridiculous' Pythagorean view - was dumped into history's rubbish heap, until Copernicus breathed new life into it and forged it into a weapon to defeat its defeaters.[40]

Imre: All pogrammes are at first only "excrescences of imagination fighting for existence by trying to outgrow each other". However, such "flowers of phantasy "must be destroyed by merciless criticism, before a single one develops further and attains some permanence". Lacking the *role of criticism*, science would be reduced to "a witches' sabbath of adventurous ideas".[41]

Paul: But my objection returns: if you don't specify a *time limit* for a (degenerating) programme, 'criticism' won't guide the growth of knowledge. How how could you then distinguish scientists doing science from witches in 'sabbath'?

Imre: It would not be very wise to assert, in the abstract, a time limit valid for all situations. In fact, any appraisal on an *individual* case should not be applied mechanically, but should rather follow from general principles allowing for

some *Spielraum*. And I chose the term 'intermittently' in my proposal to give sufficient *rational* scope for dogmatic adherence to a programme in face of *prima facie* 'refutations'. [42]

Paul: I can't tell how your idea of the growth of knowledge differs from mine. It resembles an ever-increasing *ocean of alternatives*: every single theory, every fairy-tale, every myth forces the others into greater articulation and via a competive process they all contribute to the development of our counsciousness.[43]

Imre: But I'm lucky enough to own a compass that enables me to *navigate that ocean in many ways.* I still think is useless to indicate a *time limit* in the abstract relating to a research programme, deciding, say, on its thirtieth or fiftieth degenerating version that it must be rejected. Notwithstanding, I still think it possible, sensible and practical to give that time limit *indirectly*, by comparatvely evaluating two or more programmes and their respective states of progress.

Paul: Where then does the 'objective' (as opposed to socio-psychological) reason to reject a programme lie?

Imre: The objective reason to reject a programme is supplied by *another* programme which explains the previous success of its rival and supersedes it by a further display of '*heuristic power'*.[44]

Paul: But my objection can spring back against your time limit in 'comparative terms' ...

Imre: *...* alright, but I have *no* intention of claiming any direct inference from: "programme A is currently most favored by evidence" to: "the *only rational* course of action is that of working on A" (or: "it would be *irrational* to try to develop any alternative programme"). If we were to accept this criterion, we would be claiming that all the great scientists in history have acted irrationally! The wave theory of light, for example, was not unambiguously the best theory available when Fresnel decided to work on it in the early nineteenth century; it was Fresnel's work that turned it into overwhelmingly the best available theory.[45]

Paul: This seems to me simply another way of saying that there is no *rational* way of showing that the choices taken by a scientist who works inside a degenerating research programme are necessarily irrational.[46]

Imre: You're right, and you're wrong. If we are satisfied with 'deductive rationality', then *you're right*: if we are referring to mere *logical possibilities*, then of course there is nothing illogical in believing and hoping that however badly a programme has behaved in the past, it may still recover and reach unsuspected peaks of splendour. But Duhem had already shown that deductive logic alone when coupled with crude observational results can supply only a very weak theory of rationality.[47] On the other hand, if we will not be satisfied with the weak requirements that logic alone demands from scientific practice, then *you're wrong*. No doubt there are cases of very general metaphysical ideas that have had a chequered history; once absorbed into a steadily degenerating programme, they have then much later brought back to life as elements of a progressive programme. Atomism is often cited as a good example of this. *But* if we take a look at specific cases, if we consider individual research programmes, then it becomes apparent that in the history of physics no one who has stuck to a highly degenerating research programme when a progressive alternative was available has ever managed to reverse the situation. Thus, although I agree that there is nothing *illogical* in choosing to work on a degenerating

programme, that choice is indeed *irrational* (unscientific) simply in the sense that it does not follow a *procedure* that seems to have invariably *paid off* in science.[48]

Paul: And what on earth would these 'procedures' be? The new astronomy of Copernicus, Kepler and Galileo took root; witch hunts came to an end: these facts were brought about because independent thinkers resolved to introduce and defend obsolete theories *in spite of all the traditional methodological procedures.* Witchcraft, far from being a mere outburst of folly, had a well defined structure between the 17th and 18th centuries; it was 'rationally' formulated and 'empirically' confirmed. The Copernican theory, on the other hand, contradicted some of the most convincing observations of the time, and reasonable principles of physics which had produced surprising results in physiology, psychology and even theology.[49]

Imre: I disagree and I'll explain why. It seems to me that while there has been no general agreement concerning a theory of scientific rationality, there has been considerable agreement concerning the *basic value judgements* about specific achievements of science, i.e. whether a *particular move* in the game was scientific or crankish, or whether a particular gambit was played correctly or not.[50]

Paul: And yet such 'common scientific wisdom' you give so much weight to is *not very common* and certainly *not very wise*. On the one hand, basic value judgements are not as uniform as you assume. 'Science' is split into numerous disciplines, each of which may adopt a different attitude towards a given theory, and single disciplines into schools, heresies and so forth. The basic value judgments of an experimentalist will differ from those of a theoretician. A faithful Bohrian will regard modifications of quantum theory with different eye than will the faithful Einstenian. On the other hand, basic value judgments are only rarely made for good reasons. Everyone agrees now that Copernicus' hypothesis was a big step forward but hardly anyone can give a halfway decent account of it, let alone enumerate the reasons for its ecelence. Newton's theory of gravitation was 'highly regarded by the greatest scientists', most of whom were unware of its difficulties and some of whom believed that it could be derived from Kepler's law. Whatever unity remains is dissolved during revolutions. *Revolutions* leave no theory unturned and, above all, no principle unchallenged. Now: if revolutions challenge *all* the ideas born in connection with those procedures, *including 'basic' value judgements* , how can you decide to reject, say, the standards of Aristotelian philosophy *along with* its 'basic' value judgements in order to replace them with the standards and the basic value judgements of Galileo's or Newton's science? [51]

Imre: On the basis of a 'rational reconstruction'.

Paul: On the basis of a rational reconstruction *of what*?

Imre: On the basis of the rational reconstruction of science from the point of view of *modern science*.

Paul: But in this way you are assuming what has still to be proved: the methodological supremacy of modern science. And you are also condemning the Aristotelians from 'our' point of view, without showing that *ours* is better than *theirs*.

Imre: Your position is just a colourful version of Pyrrhonian scepticism. You should look at the excellent book by Dick Popkin: *The History of Scepticism*. From a sceptic's point of view, scientific theories are a set of beliefs which have equal epistemological ranking to so many other sets of

beliefs. There may be *change* in belief systems but no *progress*. It follows that *any* system is free to grow and influence any other; but none can claim epistemological superiority. You deny any possibility of producing any theory of appraisal whatsoever. Your only piece of advice is: *do your own thing*. This is your only code of intellectual honesty.[52]

Paul: Be careful, I'm not simply a Pyrrhonian, rather a 'cultural relativist'. I think *the validity of ideas depends on the tradition against which we compare them*. Einstein is better than Newton from a modern scientist's point of view, worse from a Dinglerian's and the problem is of no interest at all from a Hopi Indian's point of view.[53]

Imre: I'm *very* careful, and to my mind there is a basic weakness in your position. I can show that you are twofaced: one face is the face of a *sceptic*, the other is the face of an *authoritarian*.[54] Let me explain: the tolerant sceptics believed that utopian dogmatism was responsible for the worst suffering of mankind. They pointed out that those who claimed to possess moral, political and religious 'truth', those who boasted of knowing which way progress lies, used the Inquisition and torture, bloody wars and genocide in order to realize their predictions in practice. Tolerant sceptics dreamt of a social contract that would decide how to restrain the human animal and minimise suffering. From their point of view - and yours - happiness and welfare replaced Truth. They argued that *betrayal of reason (or rather 'reason') by man was better than the betrayal of man by reason*. However, in the face of any controversy, a sceptic has no choice: he either turns into a dogmatist or he resorts to force without argument. Thus, ultimately, there is only one type of political philosophy consistent with scepticism: the philosophy which equates *right* with *might.* This is why many sceptics became well-paid courtiers of the bloodiest tyrants in history.[55]

Paul: I still think that scepticism *is not going far enough*. If the sceptic does not know anything, then he may well do whatever he wants to do; that is, he may engage in propaganda, he may defend the status quo, he may oppose it : 'anything goes'. Anything goes, on the other hand, clearly does not mean scepticism. It means: *anything goes*, therefore also law and order, argument, irrationalism etc. But one point has to be made clear, I would not *hurt a fly* - let alone a human being.[56]

Imre: I do not mind your anything goes, but when it comes to *moral* theory I even make mincemeat pieces of Pyrrho. Of course you would not hurt a fly, as you put it. The question is what you do when you are in a position where you can either hurt one fly or another and you are bound to hurt *one*. Would you commit suicide? You may remember that before I started on research programmes, I discovered that I had to substitute the question of the acceptance and *rejection* of theories with the *preference* of one theory to another. And this of course also applies to ethics and politics. Thus I am faced with a problem which *I* am willing to face but *you* cannot.[57]

Paul: I admit I'm wrong, and you are right. But I don't mind being wrong here and there.[58]

Imre: Well, there are two different kinds of the betrayal of reason, and yours is certainly the worst. The first consists in mitigated scepticism which originates as a sort of blind reaction to the outrages of dogmatism. It is the ancient betrayal of reason and I regard it as a venial sin. The second is radical scepticism. Undeterred by the long series of successes of Newtonian science, radical scepticism has tried to show that they were sham successes and even the best theories of the exact sciences were

nothing other than irrational beliefs. The hallmark of the modern betrayal of reason is the intellectual attack on the objective epistemological value of the exact sciences. I regard this *modern betrayal of reason as criminal.*[59]

Paul: Yours is the criminal attitude, not mine. I hold that any inquiry into a theory of 'rationality' should try to answer *two* main questions: (i) *What is science*? How does it proceed, what are its results, how do its procedures, standards and results differ from procedures, standards and results of other enterprises? (ii) *What is so great about science*? What, for instance, makes it preferable to the form of life of the Azande? What makes modern science preferable to the science of the Aristotelians? You, on the contrary, along with all the other Friends of Reason, do not show but simply assume, that modern science is 'objectively' better than the basic wisdom of witches and worlocks. In this way you take (mis-)possession of the term of 'rationality' for ideological purposes; and you equate it with the standards characteristic of a certain intellectual community: that of scientists of 'the past three hundred years'. To define as 'rational' whatever is consistent with those standards implies you have already answered to the second question*. But you have not*: you don't argue*,* you simply use the alleged superiority of science to justify those same standards you have already encoded in your methodology of scientific research programmes. [60] You are forgetting that the strength of scepticism lies in the fact that, together with the particular results, the *criteria to assess them are also changing* - you're the one who should leaf through the pages of Popkin's book![61] What would you do if faced with a 'new style of reasoning' capable of producing yet another particular knowledge? What future Lakatos will inveigh against the hypothetical-deductive method and the theory of research programmes to which it has given birth? [62]

Imre: I'm amused by your suggestion that scientific revolutions are revolutions in standards. This is of course the story I encapsuled in my announced book "The Changing Logic of Scientific Discovery".[63] I might agree that methods in science (and mathematics) change and can be expected to change. The important thing is to try to ensure that such methodological changes are for the better. However, we can take charge of this only if we succeed in rationally reconstructing change in standards as we reconstruct change in scientific theories. From this point of view my "Changing Logic" aims at grasping the "unfolding of reason" and presenting it "cut and dry", after its process of formation has been completed.[64]

Paul: But it might be the opposite of what you claim! I mean that *the better a methodology seems to capture the* rationality *of science, the greater its actual mystification of science.* After all, if the most arbitrary moves often coincide with the main radical turns in the growth of knowledge, then putting forward a 'theory of rationality' and using it to rule our ('internal') reconstruction of history is a tyrannical act of the intellect which damages both science and society. Nor is it of any use to claim stubbornly that science is superior to other forms of life. Science today reigns supreme not because of its comparative merits, but because the whole show has been rigged in its favour.[65]

Imre: Alright, alright. Putting a label with 'rational' or 'irrational' written on it to researcher's strategies is not, after all, so significant for my methodological appraisals. But I'm not waving a white flag: the fundamentally valid idea that a programme is adopted by researchers not only for its explanatory power but especially for its heuristic power, remains. It is adopted and retained for

both its ability to put on the table new and interesting problems and to point to possible solutions. And my exhortation towards a *rational reconstruction* of individual historical cases should be taken as a *historiographical* programme, an encouragement towards defining the *reasons* and *strategies* which have produced new ideas. There is, therefore, nothing wrong in appraising past beliefs according to a given norm or theory of rationality. On the contrary, such judgements lead to historical data which is not easily obtainable in other ways, and also allow one to outline and explain the whole process. This does not imply that there is any need to pry into the brains of scientists' in order to assess the 'reasons' or peculiar aversions which have governed their choices; but only that we should try to analyse and evaluate the case we are faced with *in the light of our methodological standards*. Any appraisal of 'rationality' of this kind is doubly desirable: the historian learns 'new' facts, the philosopher checks his own standards.

Paul: I'm ready to admit that as an instrument for carrying out research in the history of ideas your theory is vastly more sophisticated than Kuhn's, and so it will definitely lead to more detailed research, and to more discoveries. The discoveries may in the end turn *against you*, but that does not discredit you today, when no other theory provides an equally detailed inventory of suggestions.[66]

Imre: And I concede that one has to be sceptical with regard to an *immutable statute law*. This is why I advocate a *pluralistic* system of authority, one that would allow the particular authority of 'basic' statements to criticise the general authority of the theory of rationality, and *vice-versa*.[67] Only in this way can the proliferation of differening points of view, the comparison between different 'rational' reconstructions, the awareness of local strategies and of the reasons behind researchers' moves specify how we can *learn from history* and, especially, how we can escape from the influence of the 'worst' philosophies or, to put it in the words of John Maynard Keynes, "to emancipate ourselves from old ideas which ramify, for those brought up as most of us have been, into every corner of our minds".[68]

Paul: You've struck a blow for your side, so let me answer back in the same way, and quote from Lenin: "History in general, and the history of revolutions in particular, is always richer in content, more varied, more many-sided, more lively and ingenious than even the best parties, the most conscious vanguards of the most advanced classes can imagine. This leads to two important practical consequences. First, that in order to fulfill its task, the revolutionary class[69] must be able to master all forms or aspects of social activity without exception.[70] Second, it must be ready to pass from one to another in the quickest and most unexpected manner."[71] Lenin, of course, is addressing parties and the revolutionary vanguards rather than scientists and methodologists, but the lesson is the same: methodological rules should be adapted to the circumstances and reinvented anew each time. This increases freedom, the sense of humanity and the hope of succeeding. After all, you are the only philosopher of science who secretly imbibes the forbidden brew of *Hegelian dialectics*, and the results are evident in your magnificent work "Proofs and Refutations". All that is required now is that you confess your 'vices' openly.[72]

Imre: I'm afraid that some ardent Popperite may already be rejecting all that I am about to say, but I confess that even the *poverty of historicism* is better that

the complete absence of it. Always providing of course that it is handled with the care necessary in dealing with any explosives...[73]

Paul: ... and is placed under the right targets.

[1] See Feyerabend (1991), pp. 163-164 and *this volume*, pp. XXX.
[2] Lakatos wrote his masterpiece in the philosophy of mathematics, *Proofs and Refutations*, in dialogue form; it started from a non-problematical situation and gradually evolved into BETA's final remark: "I had no problems at the beginning, and now I have nothing *but* problems!".
[3] See Feyerabend (1973) and (1975a), ch. 16.
[4] Lakatos expresses his worry that philosophers, or theologians, or party civil servants, or, indeed, any kind of religious or state authority, in attempting to frame scientific research in preordained guidelines, are in fact responsible for its sluggishness. See the Lecture I, *this volume*.
[5] Feyerabend, *this volume*, p. XXX.
[6] Lakatos (1970) claims that "there are no such things as *crucial experiments*, at least if these are meant to be experiments which can istantly overthrow a research programme." The idea of "*instant rationality*" is thus utopian. We may claim an experiment crucial only "*with long insight*", when one programme suffers defeat and it is superseded by another one: "Kepler's ellipses were generally admitted as crucial evidence for Newton and against Descartes only about one hundred years after Newton's claim. The anomalous behaviour of Mercury's perihelion was known for decades as one of many yet unsolved difficulties of Newton's programme; but only the fact that Einstein's theory explained it better transformed a dull anomaly into a brillant 'refutation' of Newton's research programme." The rationality of science is therefore slower than most people tend to think, like "Minerva's owl, it flies at dusk" (pp. 72, 86-87.)
[7] See Lakatos (1968a), p. 343. Lakatos' view on the pragmatic relevance of theological and philosophical speculation comes out expicitly in Lecture I, *this volume*, where it is claimed that the demarcation between science and pseudoscience is not a problem of armchair philosophy, but is of vital social and political importance.
[8] See Lakatos (1971a), p. 112 and (1970), p. 70.
[9] See Feyerabend (1970c), p. 215 and (1976a), p. 214.
[10] See Lakatos (1971b), pp. 174, 178. On the different positions held by Lakatos concerning the link between appraisals and advice, see Motterlini (1993).
[11] Cf., for example, Popper (1934/1959), chapters I and II; see also Lakatos (1971a), p. 103.
[12] See Musgrave (1978), p. 475.
[13] See Feyerabend (1970c), pp. 215-216.
[14] See Lakatos (1971a), p. 117.
[15] Cf. Hall (1971), p. 152 and Musgrave (1978), p. 487.
[16] See Feyerabend (1976a), p. 216.
[17] See Lakatos (1971a), p. 117.
[18] See Feyerabend, *this volume*, pp. XXX, (1975a), p. 197 and (1976a), p. 213.
[19] See Feyerabend (1978a), p. 185.
[20] See Feyerabend, *this volume*, p. XXX, (1975a), pp. 187-200.
[21] See Feyerabend, undated letter to Lakatos, *this volume*, p. XXX.
[22] See Lakatos, reply to Feyerabend, 25 January 1973, *this volume*, p. XXX.
[23] Cf. Hacking (1979), p. 169.
[24] See Lakatos (1974a), pp. 154-159.
[25] The remark here attributed to Feyerabend is actually Newton-Smith's (1981), p. 68.
[26] See Lakatos (1971a), p. 101.
[27] See Lakatos (1968a), 181-191 and Worrall (1978b), p. 326.
[28] Reference is obviously to the problem of the justification of inductive inferences which, along with "Kant's problem" (also known as the problem of demarcation) is, according to Popper (1932-1933/1979) the fundamental problem of the theory of knowledge.
[29] See Lakatos (1974a), p. 158. Popper would seem finally to admit this in his *Replies to Critics*: "there may be a 'whiff' of inductivism [which] enters with the vague realist assumption that reality, though unknown, is in some respects similar to what science tells us, or, in other words, with the assumption that science can progress towards greater verisimilitude. (1974, p. 1193).
[30] Mach (1905), p. 171. Lakatos does not quote him directly but puts forward a similar idea in his (1974a), p. 158.
[31] See the whole chapter 14 ("The Hypothesis") in Mach (1905). For an evaluation of Mach far from the positivistic cliché, see Feyerabend (1987), chapter 7.
[32] See Lakatos (1968a), pp. 186-187
[33] See Feyerabend (1993), p. 21 and this volume, pp.xxx..
[34] Evidently, this is not Lakatos, but the New Testament, *John*, VIII, 32.
[35] See Feyerabend (1973).
[36] This is not Feyerabend but Italo Calvino, *Six Memos for the Next Millennium*, Vintage, London, p. 12.
[37] See Feyerabend, letter to Lakatos, April 1971, *this volume*, p. XXX.
[38] See Feyerabend (1980a), ch.11.
[39] See Lakatos (1970), p. 49.
[40] See Feyerabend (1975a), p. 41.
[41] Mach (1905), p. 77.
[42] See Lakatos (1970), p. 49.
[43] See Feyerabend (1993), p. 21.

[44] That is to say for the ability of the programme to anticipate "novel" facts during its growth, see Lakatos (1970), p.69. On the notion of the "heuristic power" of a programme, see also Urbach (1978) and Will (1992). On the concept of "novelty", see Worrall (1978a) and (1985), Zahar (1973), and also *this volume*, pp. xxx.
[45] See Worrall (1990), p. 332.
[46] See Feyerabend (1976a), p. 389.
[47]Cf. Duhem (1906): "*Pure logic is not the only rule for our judgements*; certain opinions which do not fall under the hammer of the principle of contradiction are in any case perfectly unreasonable. These motives which do not proceed from logic and yet direct our choices, these 'reasons which reason does not know' and which speak to the ample 'mind of finesse' but not the 'geometric mind', constitute what is appropriately called good sense […]. After Foucault's experiment had shown that light traveled faster in air than in water, Biot gave up supporting the emission hypothesis; strictly, *pure logic would not have compelled him to give it up*, for Foucault's experiment was not the crucial experiment that Arago thought he saw in it, *but* by resisting wave optics for a longer time Biot would have been *lacking of good sense*." (pp. 218-218, emphasis added)
[48] See Worrall (1990), pp. 346, 349, 350. Worrall claims further that such a "stronger" theory of scientific rationality "would have nothing resembling an a priori justification, but would simply rest on an inductive extrapolation from past to future - this time an inductive extrapolation of a methodological kind" (ibidem, p. 46). Cf. *above* the Popperian 'solution' of the problem of induction advocated by Lakatos.
[49] See Feyerabend (1975a), chapters 6-12.
[50] See Lakatos (1971a), p. 124.
[51] See Feyerabend (1976a), pp. 207-210.
[52] See Lakatos, *this volume*, pp. XXX, and (1973), pp. 107-108.
[53] See Feyerabend (1978a), patrt II, ch. 3 and (1987). On Feyerabend's relativism see also Couvalis (1989) Appendix 1.
[54] See Lakatos, letter to Feyerabend, 2 March 1973, *this volume*.
[55] See Lakatos, "The Intellectuals' Betrayal of Reason", *this volume*, Appendix 3.
[56] See Feyerabend, letter to Lakatos, 7 August 1972, *this volume*.
[57] See Lakatos, replies to Feyerabend, 14 August 1974, *this volume*.
[58] See Feyerabend, letter to Lakatos, August 1972, *this volume*.
[59] See Lakatos, *this volume*, Appendix 3.
[60] See Feyerabend (1975a), p. 181, (1976), p. 203, and (1978a), pp. 99-107.
[61] The reference is here to Popkin's *History of Scepticism from Erasmus to Spinoza* (1964).
[62] Cf. Hacking (1979), pp. 185-187.
[63]See the letter dated 1oth January 1974, *this volume*. "The Changing Logic of Scientific Discovery" is the title of the book Lakatos had in mind to write, but which was never completed, see also *this volume*, pp. XXX.
[64] Note the Hegelian language. Together with Popper's critical philosophy, Hegel's dialectic was indeed one of the major "idealogical sources" of Lakatos's philosophy. On a few possible ways in which Lakatos might have carried out his projected "Changing Logic of Scientific Discovery", see Worrall (1988) and (1989), Kadvany (1995), Larvor (1998), pp. 205-210, Motterlini (1999).
[65] See Feyerabend (1978a), p. 103.
[66] See Feyerabend (1975c), p. 17. Feyerabend was not alone in thinking this: many were the case studies inspired by the methodology of scientific research programmes since then, both in the social and the natural sciences; cf. *this volume*, p. XXX.
[67] See Lakatos (1971a), pp. 136-138.
[68] Keynes, *The General Theory of Employment, Interest and Money*, MacMillan, London 1973, p. xxiii.
[69] I. e. the class of those who want to to change either a part of society such as science, or society as a whole.
[70] I. e. it must be able to understand, and to apply, not only a particular methodology, but any methodology, and any variation it can imagine.
[71] Lenin, "Left-Wing Communism" - An Infantile Disorder", *Selected Works*, Vol.3, London, 1967, p. 401. Quoted by Feyerabend (1975a), pp. 17-18.
[72] See Feyerabend (1970a), p. 101.
[73] See Lakatos (1959-1961), p. 61. For an overall assessment of Lakatos's philosophy "between the Hegelian Devil (i.e. historicism) and the Popperian deep blue sea (i.e. fallibilism)", see Motterlini (1999).