# Pera, Marcello

## Narcissus at the Pool : Scientific Method and the History of Science

*Marcello Pera* (Italy)

# NARCISSUS AT THE POOL:
## SCIENTIFIC METHOD AND THE HISTORY OF SCIENCE*

### INTRODUCTION

Once upon a time there was a kingdom whose inhabitants practiced and propagated a religion that may be called the religion of *science as demonstration*. It was part of that religion to view science as a cognitive activity which, if properly pursued, reaches truth and certainty, and progresses by adding the new truths to the old ones. The Pope of that religion was called Scientific Method; the inhabitants of the kingdom venerated him, for they thought that, thanks to his infallible guidance, they could reach their aims, avoiding any substantive controversies or solving them in a neutral, impersonal way. They also thought that, without the authority of their Pope, science would collapse into an irrational enterprise. In this vein, an influential leader of the kingdom, called Descartes, put forward the idea that when two people disagree over the same thing we may decide, thanks to "certain and easy rules", that one or both of them is wrong and does not possess science at all. And in the same vein a successor of that leader, Sir Karl Popper, maintained that scientific method offers a "clear line of demarcation" between science and pseudo-science or metaphysics.

As is well known, under the attack of a band of disbelievers, a revolution took place in our kingdom. The authority of the Pope was ridiculed with the slogan "Anything goes", the dogma that science reaches truth was criticised

and proved to be untenable, and the dogma that science progresses was shown to be unrealistic. Step by step, the old religion was broken up and finally replaced by the new creed of *science as propaganda*.

Not everybody accepted to be driven out of the temple. Many philosophers of science admitted that the idea of a single, comprehensive, clear-cut set of *regulae ad directionem ingenii* or of *regulae philosophandi* is a myth and they came to recognize that there exist many different methods, each of them capable, actually or in principle, of promoting scientific research. Other philosophers also granted that the variety of scientific practice, as witnessed by the history of science, is such that it does not tolerate any sharp demarcation criteria, and some of them even drew the conclusion that the problem of demarcation itself is misconceived. Yet they did not give up the idea that science is a rule-governed activity; rather they took the proliferation of methods as an historical fact and as challenge to methodology. Accordingly, the new problem they were faced with was whether different methods can be compared and how one of them can be selected as the best or the most efficient or the most rational.

A widespread view, which may be labelled "historicist methodology" (or meta-methodology), holds that such a comparison is possible and the selection should be made in historical terms. The history of science—this view maintains—is the laboratory of methodology; it provides crucial tests of rival methodological claims in the same way in which empirical evidence provides tests of rival scientific hypotheses.

The aim of this paper is to discuss and discard this view. I shall argue that, although history may play a role, it does not solve the problem of choice between methods. Many other factors—personal, social, cultural factors—besides history, enter into our choice. This is not to say that, owing to these pragmatic factors, science loses its rationality, nor do I maintain that the idea of method is only a "verbal ornament". If the religion of science as demonstration is untenable, the creed of science as propaganda is equally misconceived. The last part of this paper purports to suggest that the image of *science as argumentation* is a more realistic and promising picture of scientific practice.

But before entering into the details of historicist methodology, I wish to outline a few general reasons for my scepticism in this kind of undertaking.

### 1. CONSOLATION FOR THE METHODOLOGIST

In his famous "Towards an Historiography of Science", Joseph Agassi complained that the history of science was in a "lamentable state", due above all to "the naive acceptance of untenable philosophical principles",[1] namely,

---

[1] J. Agassi, "Towards an Historiography of Science", in: *History and Theory*, Supplement 2, Mouton, Hague 1963; see pp. V and VII.

according to Agassi's own diagnosis, the inductivist and conventionalistic philosophies. As a remedy, he proposed that the wrong principles should be replaced by Popper's critical philosophy of science.

I am not sure the picture was so black nor that it is any better now, and, supposing it is, I am not sure that the improvement can be ascribed to the remedies proposed by Agassi.[2] Even granting that those collections of facts awkwardly made up into a modest inductivist package were bad history (which sometimes was really the case), I am afraid that those "rational reconstructions" so popular with philosophers today are not better history. I also fear that the mutual relationships between historians and philosophers of science have not been getting any more peaceful.

This may be due to several factors. Perhaps the medicine prescribed by Agassi was too strong and it will take time for it to act on the body of historians, debilitated by years of inductivist malpractice. Or perhaps the very kind of therapy was wrong and dangerous. Although I personally favour the second hypothesis, I will not go into this matter here. Rather, I shall deal with the main thesis underlying Agassi's complaint, and especially with its symmetric thesis.

Agassi's main view was that the history of science depends upon the philosophy of science. Tell me which philosophy of science you profess and I shall tell you whether the history of science you practice is creditable. The symmetric view, which Agassi did not elaborate upon and with which perhaps he does not agree but which is very widespread today, is that the philosophy of science must be assessed on the basis of the history of science. Tell me what sort of history of science you reconstruct and I shall tell you whether the philosophy of science you profess is to be maintained. Amusingly enough, while Agassi was suggesting that the history of science should be improved with the help of Popper's philosophy of science, Feyerabend and Lakatos were beginning to discredit Popper's philosophy of science with the help of the history of science. The cunning of history or the lag of philosophy? We do not know. What we do know instead is that Agassi's main view and the symmetric view have been widely accepted and have given rise to two distinct but related programmes, namely a philosophical history of science and a historical philosophy of science, as we may call them. According to me, both programmes contain a mortal danger.

The danger of philosophical history of science is to reduce history to philosophy taught through examples, to use a famous phrase. I feel that such a danger has partly become a reality and, in the context of the dependence view,

---

[2] For a critical discussion of Agassi's view, see M. Finocchiaro, *History of Science as Explanation*, Wayne State University Press, Detroit 1973. Finocchiaro rightly maintains, in my view, that the history of science does not depend on the philosophy of science in the sense stated by Agassi, and that it would not be improved by substituting "critical" for "naive" philosophies of science.

this is inevitable. A history of science reconstructed on a philosophy of science is bad history, even though the philosophy is good. It is doomed to finish like thesis art, which soon degenerates into irritating propaganda, even though the cause may be noble and edifying; or to finish like novels with a happy ending, or, if you prefer, like psalms that always end with a Gloria Patri.

The danger of historical philosophy of science, by contrast, is to reduce philosophy to history submitted to precepts. I am afraid this danger, too, has not been avoided and is indeed inevitable. Every philosophy that relies upon history ends up, like Narcissus looking into the pool, by looking at its own reflection in the water and falling in love with it. The fact is that history is a generous and consoling lady; she responds to Popperians, Lakatosians, Laudanians, etc., in exactly the same way. Generally, she responds to all who turn to her for suggestions or moral precepts; for this reason she loves anarchists and, in her turn, is loved by them; because she says yes to all and is faithful to none.

It is not my intention to say that this must necessarily be so. We might think of a nonconsoling use of the history of science in the same way in which Popper suggests to make a nonconfirming use of empirical evidence.[3] Nor do I maintain that the history of science is of no help to the philosophy of science. On the contrary, if we aim at a realistic image of science, we must admit that history is an essential source of information, although it is not the only one. Yet the fact that historical reconstructions and tests infallibly confirm those methodologies in terms of which they have been made, just as the pool confirms to any Narcissus that he is the best creature in the world, should induce us to suspect that this kind of undertaking is doomed to failure. More specific reasons will be given later. The moral I draw is that an amicable separation between the history and the philosophy of science, as far as methodology is concerned, is a good policy to pursue.[4] In my view, this separation helps history, for it defends it from the risks of history-fiction; it also helps philosophy for it prevents it from the risks of a new kind of reductionism. When I was learning the tricks of the trade as an undergraduate I was taught that philosophy is logic; now that I have grown up with the cult for logic, I am

---

[3] A taxonomy of the relationships between the philosophy and history has been proposed and discussed by R. Burian, "More than a Marriage of Convenience: On the Inextricability of History and Philosophy of Science", *Philosophy of Science*, 44, 1977, pp. 1–42. My criticism does not apply to Burian's view (with which I agree) that "specifically historical considerations affect the determination of the degree of support for a given theory at a given time"; however, I do not think that "historical studies are of considerable importance in evaluating philosophical claims about the logic of support", if this has to be taken in the sense that the history of science is a test of norms of evaluation.

[4] Many arrangements have been advocated in this context; see R. Giere, "History and Philosophy of Science: Intimate Relationship or Marriage of Convenience?", *British Journal for the Philosophy of Science*, 24, 1973, pp. 282–297; E. McMullin, "History and Philosophy of Science: A Marriage of Convenience?", *PSA* 1974, edited by R. S. Cohen et al., Reidel, Dordrecht–Boston 1975, pp. 515–531.

told that philosophy is history. While still appreciating logic and serving my apprenticeship with history, I hope the time is nigh when philosophy will be nothing but philosophy.

My plea for separation will be divided into three parts: first I shall argue for the relevance of the problem of method, I shall then analyze the main flaws in historicist methodology and, finally, I shall outline an axiological vindication of method. It is at this point that I shall propose the idea of science as argumentation.

## 2. THE RELEVANCE OF THE PROBLEM OF METHOD

The central idea of historicist methodology is that "all methodologies function as historiographical (or meta-historical) theories (or research programmes) and can be criticized by criticizing the rational historical reconstructions to which they lead"; in this way, "history may be seen as a test of its rational reconstruction".[5] Sometimes, in the same role, history is replaced by psychology, biology or physics,[6] but I shall not make this distinction because what I have to say applies equally to both attempts to reduce methodology to an empirical discipline. It should be pointed out, however, that, here, methodology will be understood as a theory of method taken in the ordinary sense of conceptual rules or norms of inquiry and not of operative techniques to accomplish specific moves in scientific inquiry. In this latter sense, an empirical methodology would be trivially true but philosophically irrelevant, for techniques are instruments and the setting up and checking up of instruments are not the job of philosophy but constitute an integral part of scientific activity.

Historicist methodology comes in two versions, one optimistic and the other pessimistic, which correspond to the two possible outcomes of its justificatory procedure. Indeed, when we resort to history, we may find either that

(1) the history of science shows at least one specific epistemic model or pattern invariant through all, or through the main, episodes; or that

(2) the history of science shows no particular epistemic model or pattern. Correspondingly, optimistic historicist methodology maintains that

(1′) the preferable (or most rational, etc.) method is the one which best fits the epistemic patterns of history;

whereas pessimistic historicist methodology maintains that

(2′) no scientific method exists and our search for it is useless.

---

[5] I. Lakatos, "History of Science and its Rational Reconstructions", *PSA* 1970, edited by R. Buck and R. S. Cohen, Reidel, Dordrecht — Boston 1971, pp. 91 — 136; see p. 109. See also L. Laudan, *Progress and its Problems*, University of California Press, Berkeley 1977, p. 162; "the authentication of any philosophical model requires careful research in $HOS_2$".

[6] This is Laudan's recent view, see his *Science and Values*, University of California Press, Berkeley 1984, pp. 39 — 40ff.

Sometimes historicist methodology upholds a third, additional thesis, namely that

(3) scientific method has no heuristic strength.

Elsewhere,[7] I have raised doubts about (1) and expressed my inclination for (2). I now intend to argue that, even if (1) is true, it does not entail (1') and that, in the same way, (2) does not entail (2'). Moreover I intend to show that (3) is untenable. I shall begin with (2') and (3).

(2') is notoriously Feyerabend's view, according to which the lowest common denominator we can draw from the history of science is the rule that "anything goes". An alternative way of expressing (2')—or, better, a consequence of it, if we consider the main function traditionally ascribed to method—is saying that there does not exist any demarcation criterion that may fit the history of science and that the problem of demarcation itself is spurious.

Larry Laudan, for instance, upheld this view using an argument that is typical of pessimistic historicist methodology. "The evident epistemic heterogeneity of the activities and beliefs customarily regarded as scientific should alert us to the probable futility of seeking an epistemic version of a demarcation criterion. When, even after detailed analysis, there appear to be no epistemic invariants, one is well advised not to take their existence for granted. But to say as much as is in effect to say that the problem of demarcation—the very problem that Popper labelled 'the central problem of epistemology'—is spurious, for that problem presupposes the existence of just such invariants."[8]

In my opinion, this argument is a *non sequitur*. First, the fact (let us grant it is a fact) that the history of science does not exhibit a single or invariant demarcation criterion does not imply that no criteria at all exist: we might think that there are local or partial criteria. And secondly, the fact that in the history of science or in the current application of the term "science", no (nongeneric) epistemic invariants can be found, does not imply that the problem of demarcating genuine science from pseudo-science is spurious. This would be the same as saying that the fact (and this is really a fact) that many sins and all kinds of violations to all possible moral codes exist and have always existed implies that the problem of good and evil is spurious. The argument would only be valid if the problem of the demarcation criterion were simply a problem of description of uses: in this case, the difficulty of detecting invariants might serve to discourage further attempts or to arouse the suspicion they are useless. But the purpose a demarcation criterion, as well as of any methodological rule, is not so much to describe what has happened as, rather, to assess what has happened and influence what will happen. And this two-fold purpose cannot be given up.

[7] See my "In Praise of Cumulative Progress" in *Change and Progress in Modern Science*, edited by J. Pitt, Reidel, Dordrecht 1985, pp. 267—282.

[8] L. Laudan "The Demise of the Demarcation Problem", in *Physics, Philosophy and Psychoanalysis*, edited by R. S. Cohen and L. Laudan, Reidel, Dordrecht—Boston 1983, pp. 111—127, see p.124.

Laudan himself admits that "it remains important to retain a distinction between reliable and unreliable knowledge", and, it may be argued, it remains important to search for a reliability criterion. But if a criterion of epistemic reliability is important, it is so for exactly the same reasons for which the criterion of demarcation was important; the two criteria have the same function, although they may give rise to nonoverlapping partitions of the universe of cognitive claims. Thus, the fact that, when the reliability criterion is available, "the class of statements falling under that rubric will include much that is not commonly regarded as 'scientific' and it will exclude much that is generally considered 'scientific'",[9] merely proves that an old criterion has been replaced by a new, wider one, not that the problem is spurious. If Laudan judges the relevance of problems of this kind on the basis of the history of science, then exactly the same objections he raises over the demarcation criterion can be used over the reliability criterion which he advocates should be searched for: why should the history of science be more generous with the one than with the other?

Laudan's criticism is effective if addressed not against the problem of demarcation as such but against the solutions philosophers have put forward to distinguish science from pseudo-science. He is quite right in saying that "none of the criteria which have been offered thus far promises to explicate the distinction"; but the most pertinent conclusion to be drawn from this undeniable failure is not that the problem is spurious but that the kind of solution traditionally looked for is impossible. Now that solution was part of the religion of science as demonstration; its unattainability is then a good argument to change the received religion, not to lose the faith in the problem.

But it is not just the problem of method, as a source of value judgements, which is important; so is the problem of heuristics, too. Heuristics is the other aspect of method, because method cannot be retrospective only. A criterion of good and evil is needed and used not only to evaluate the actual or past moral behaviour, but also to suggest what remedies should be used to improve it. In a similar way, a method or a criterion of what is scientific or rational is needed not only to evaluate the cognitive behaviour of people, but also to suggest what ought to be done, for example, what theory ought to be chosen to make the choice scientific or to be preferred to make it rational or progressive. Lakatos is right to distinguish between value judgements (such as "*x* is good") and ought--statements (such as "you must do *x*").[10] Indeed, the former do not entail the latter. But to know that something is good and to disregard it, which is a well-known fact of moral behaviour (*video meliora proboque sed deteriora sequor*), is nonetheless felt to be an inconsistency, and this proves that a bridge, a link, exists between the two kinds of judgement.

Originally, Lakatos, too, thought that "moral standards, by which one

---

[9] *Ibid.*, p. 125.
[10] See I. Lakatos, *op. cit.*, p. 123 ff.

judges people, have pragmatic implications for education".[11] If he changed his mind later, this was due to serious difficulties of principle which affect historicist methodology. To these difficulties we turn now our attention.

### 3. THE FLAWS OF HISTORICIST METHODOLOGY

Let us consider a method $M$. There are two main ways in which $M$ can be compared with the history of science ($HOS$) and, correspondingly, there are two main kinds of historicist methodology. The first way consists in inferring $M$ directly from $HOS$ and selecting that $M$ which is most instantiated; in this case we make an inductive use of $HOS$ and we may speak of inductivist historicist methodology, for it proposes to learn from (historical) evidence taken as a secure, incorrigible, starting point. The other way consists in setting out in advance one or sveral $Ms$ and retaining that $M$ which has best withstood the tests of $HOS$; in this case, we make an hypothetico-deductivist, or experimental or heuristic, use of $HOS$ and we may speak of hypothetico-deductivist historicist methodology. In this Section I shall focus on the former way.

In Lakatos' methodology, the starting point is a special class of value judgements that are documented in the annals of $HOS$; these judgements express the "basic appraisals of the scientific élite" or the "verdicts of the best scientists",[12] at least of the "last two centuries". The more an $M$ fits these verdicts the better it is. Laudan proposes a similar, but more sophisticated, solution. Elaborating on a hint from Lakatos, he distinguishes between $HOS_1$, that is, "the chronologically ordered class of beliefs of former scientists", and $HOS_2$, that is, the "descriptive and explanatory statements which historians make about science",[13] and he proposes that $M$ be compared with $HOS_1$. More precisely, Laudan suggests $M$ be compared with that subclass of preanalytic intuitions $PI$ of $HOS_1$, which contains "cases of theory acceptance and theory rejection about which most scientifically educated persons have strong (and similar) normative intuitions".[14] For Laudan, too, the $PIs$ are "decisive touchstones for appraising and evaluating different normative models of rationality"; the best $M$ is the one that most accurately fist these $PIs$.

In order to examine this view, we must have a clear idea of just what is

---

[11] I. Lakatos, "Changes in the Problem of Inductive Logic", in I. Lakatos (ed.), *The problem of Inductive Logic*, North Holland Publ. Co., Amsterdam 1965, p. 343. On Lakatos' separation of methodology from heuristics, see P. Quinn, "Methodological Appraisal and Heuristic Advice: Problems in the Methodology of Scientific Research Programmes", *Studies in History and Philosophy of Science*, 3, 1971, pp. 135–149; A. Musgrave, "Method or Madness?", in *Essays in Memory of Imre Lakatos*, edited by R. S. Cohen et al., Reidel, Dordrecht–Boston 1976, pp. 457–491.

[12] I. Lakatos, "History of Science etc.", *op. cit.*, pp. 111, 121.

[13] L. Laudan, *Progress and its Problems*, *op. cit.*, p. 158.

[14] *Ibidem*, p. 160.

being compared with what. It may then be useful to introduce the following kinds of assertions:

$\gamma$ = subjective and particular value judgements. These are value judgements which mention the individual who expresses them and which refer to a specific object, for instance, "(Einstein, 1905): it is rational to accept the special relativity theory."

$j$ = objective and particular value judgements. These are value judgements which do not mention individuals but still refer to a particular object, for example, "It was rational to accept the special relativity theory in 1911."

$J$ = objective and general value statements. These statements neither mention the individual who expresses them nor refer to a specific object; for example, "It is rational to accept well-confirmed theories."

$\Gamma$ = normative statements. These are statements which express imperatives or commands; for instance, "Accept well-confirmed theories." [15]

Let us now compare $J$ with $j$, as Laudan suggests. In my view, this procedure encounters the following main flaws.

First. The comparison is circular. A $j$-statement is not the algebraic sum nor the logical consequence of $\gamma$-statements. The fact that most, or even all, members of a community were of the opinion, at a given time $t$, that a certain theory $T$ should be accepted does not imply it was rational to accept $T$ at $t$. After all, those individuals might have been wrong; they might have been mistaken in examining the merits of $T$ or they might have been using bad reasons. Rather, a $j$-judgement is an historical evaluation resulting from, and depending on, an analysis of the cognitive situation at $t$, plus a $J$-judgement as to what, in general, it is rational to accept. So, to say it was rational to accept Copernican theory in 1839, when Friedrich W. Bessel discovered stellar parallax, rather than in 1610, at the time of Galileo's telescopic observations means sharing the general value judgement that it is rational to accept theories well-confirmed by empirical evidence. But if a $j$-judgement depends on a previously accepted $J$-judgement, then it belongs to $HOS_2$, thus, to compare $J$ with $j$ would be, according to Laudan's own view, like comparing $J$ with itself.

The second flaw of this procedure is that:

The comparison is not decisive. This is clear from the fact that a $j$-judgement is compatible with several $J$-judgements differing from one another. Let us consider the case of the special relativity theory. Almost all historians agree that, in 1911, at the time of the first Solvay Conference in Brussels, practically all the leading scientists accepted the theory, or at least the formalism of the theory. But when we pass from this simple statistical assertion to the more demanding $j$-judgement "It was rational to accept the special relativity theory in 1911", we find that historians, like scientists, are divided.

---

[15] This conceptual apparatus may obviously be found in the vocabulary of historicist methodology. $\gamma$s are Lakatos' "basic appraisals of the scientific élite"; $j$s are Laudan's "preanalytic intuitions"; $J$s are Lakatos' "value judgements" and $\Gamma$s are Lakatos' "ought statements".

Some think that, in 1911, it was rational to accept Einstein's theory because it had great heuristic strength; others are of the opinion that it was rational to accept it because of its empirical confirmations; others, again, adduce still different reasons. This obvious fact is no cause for scandal; it is, however, cause for problems for those who wish to compare a $J$-with a $j$-judgement and to assess the former on the basis of the latter. Such a comparison is clearey not decisive because not only $j$-judgements depend on a single $J$-judgement but may also depend on many different and even contrasting $J$-judgements. Thus, what look like obvious "pre-analytical intuitions" turn out to be problematical historical evaluations which, far from being "decisive touchstones of comparison", may become, more or less, a source of consolation for almost every method.

The third flaw of the comparison between $J$ and $j$ reinforces this conclusion. The comparison is arbitrary. $j$-judgements are not like the Ten Commandments; they do not emanate from a supreme authority, but spring from numerous sources. In the field of the history of science there are influential leaders but no Moses. Now, since these leaders do not necessarily agree among themselves, the $j$-judgements they express will, as a rule, be heterogeneous. The situation is the opposite of the one already examined: not only a single $j$-judgement is compatible with more than one $J$-judgement, but a single $J$-judgement may allow more than one $j$-judgements. For example, two historians both convinced that it is rational to accept well-confirmed theories, may, nevertheless arrive at different $j$-judgements as to whether it was rational to accept, say, Copernican theory in 1633. For, as we have seen, these judgements also depend on an historical analysis of the cognitive situation. We should then ask, How is the class of the basic $j$-judgements formed? or How is the class of the most influential historians selected? These are, obviously, crucial questions, because different classes of pre-analytical intuitions or different classes of authorities may support different epistemic patterns and different methods. Now, if we say that these classes are formed on the basis of a criterion, such a criterion cannot be but a $J$-judgement and the comparison between $J$ and $j$ therefore ends up being circular; if we say that there is no criterion, the comparison turns out to be arbitrary and ineffective, for any method could be compared with, and consoled by, any historical judgement.

We find the same defects if we compare $J$ and $\gamma$ directly, as Lakatos suggests. In this case, the situation is even less favourable for historicist methodology.

In the first place, a $\gamma$-judgement, such as Einstein's judgement in 1905, "It is rational to accept the special relativity theory", may be used to give support to a $J$-judgement, for instance the judgement "It is rational to accept theories with a strong heuristic power", only if we believe that what Einstein did was really rational, but this presupposes the very judgement that it is rational to accept theories with strong heuristic power. We are thus spinning in a circle: a $\gamma$-judgement supports the $J$-judgement which it presupposes.

Secondly, a $\gamma$-judgement does not uphold one $J$-judgement only, but several $J$-judgements because the reasons underlying a scientist's personal assessment may be numerous, conflicting and not always recommendable. As Feyerabend rightly puts it, "basic value judgements are only rarely made for good reasons".[16] Einstein thought it was rational to accept the special theory of relativity in 1905 for reasons of symmetry, simplicity and elegance; R. C. Tolman and G. N. Lewis accepted it in 1908 because of the empirical evidence; W. Wien accepted it in 1909 for reasons of intrinsic coherence; etc. What $J$-judgement can be drawn from, and is most compatible with, these "basic appraisals of the scientific élite?"

Thirdly, for each $\gamma$-judgement there is, as a rule, a $\gamma$-judgement contrary to it. The history of science contains both "(Einstein, 1905): it is rational to accept the theory of relativity" and "(Lorentz, 1905): it is irrational to accept the theory of relativity". On what ground should we decide that the first judgement and not the second is the genuine touchstone of comparison? Because Lorentz was in a minority? This would obviously be begging the question. There must be a criterion. But if a criterion exists, we are in a circle once again, if it does not exist, almost everything can become a touchstone of comparison and a source of consolation.

All this has fatal effects on $\Gamma$-statements which properly express methodological rules. We are taken into the horns of a dilemma: either $\Gamma$-statements are independent from $J$-judgements, as Lakatos maintained, and then inductivist historicist methodology does not offer any solution to the problem of choice between rival methods, for it merely reduces to registering that such and such method has been successfully used in such and such cases, but it leaves us free to use it again in the future. Or $\Gamma$-statements are dependent on, or linked to, $J$-judgements, and not even in this case can historicist methodology solve the problem of choice, for the ascertained historical variety of $J$-judgements leaves us with a superabundance of rules.

These objections not only show that the history of science does not supply univocal methodological indications. There is something more. Whatever is supplied by the history of science does not lend normative strength to methodological rules. In the field of ethical systems, the objection has been known ever since Socrates: is the holy holy because the gods like it, or because it is holy do the gods like it? The situation is the same in the field of methodological systems: is a given rule or norm, for instance the rule of accepting well-confirmed theories, rational because Galileo, Newton and Einstein accepted it, or did Galileo, Newton and Einstein accepted it because it is rational? In the first case, "ought" is inferred from "is" and we get involved in

---

[16] P. Feyerabend, "On the Critique of Scientific Reason", in C. Howson (ed.), *Method and Appraisal in the Physical Sciences*, Cambridge 1976; also published, under the title "The Methodology of Scientific Research Programmes", in P. Feyerabend, *Philosophical Papers*, 2 vols., Cambridge 1981, vol. 2, p. 209.

the naturalistic fallacy; in the second, "ought" is properly used to assess what is or has been, but the criterion of assessment cannot be based on history.

This is not to say that the history of science is irrelevant. On the contrary, it provides us with the best source of information about what methods are used and what goals are reached. The current historical philosophy of science has the advantage over the previous logicistic approach that it makes us familiar with a growing number of real episodes, instead of "logical substitutes" which never existed either on earth or on heaven. The historicist methodology, however, is wrong when it transforms what, at the very most, are *examples* of rational behaviour into *grounds* or *warrants* of rationality. We cannot say a method *M* is good because it saves the history of science *HOS* wholly or partly; we may say, rather, that if *M* is good, then that part of *HOS* it saves is also good. We have always to be able to correct even the judgements of the gods, firstly, because even the gods make mistakes and, secondly, because even the gods quarrel, but it would not be possible to correct the errors of the gods if we take their desires, and maybe their whims, too, as dogmas.

The weakness of historicist methodology is even more evident if we consider normative judgements. The fact that some or many leaders of the scientific community have, for example, accepted well-confirmed theories does not give support to the imperative that in science only well-confirmed theories must be introduced and accepted. It is not the examples of history that oblige us or suggest what we should do; obligations and suggestions may only stem from our decision to imitate them, together with our sincere and critical appraisal that they are genuine examples of values worth pursuing.

This conclusion suggests we should take the opposite road to that of historicist methodology. I shall call this road axiological vindication of method and I shall attempt to give a brief, general outline of it. This will lead us to examine also the hypothetico-deductivist use of the history of science.

## 4. AXIOLOGICAL VINDICATION OF METHOD

The first thing to do is to specify certain demands. A justification of a scientific method *M* (or of a particular rule of method *m*) will be considered satisfactory if *M* (or *m*) satisfies at least the three following requisites:
    (a) it saves past or present scientific practice at least partly;
    (b) it possesses normative strength and heuristic power;
    (c) it is sufficiently precise.
With the exception of the controversial question of heuristic strength, these requisites are accepted by the upholders of historicist methodology. Laudan, for instance, imposes them upon the demarcation criterion and, presumably, on the reliability criterion.[17] They are also accepted by the supporters of the

---

[17] See Laudan, "The Demise ...", *op. cit.*, p.118.

so-called "Euclidean" or *a priori* methodologies. In particular, Popper himself suggests, with reference to (a), that his definition of empirical science and his proposal of scientific method are, to a certain extent, assessed by scientific practice and corroborated by the history of science.[18]

Since scientific practice, even in its best exemplars, seems to respond to any kind of requests and looks messy enough to give the picture of a labyrinth rather than of a well-ordered building, we are warned from the outset that it is no easy task to satisfy requisite (a). Two extreme positions are, of course, to be avoided here. We should not stipulate a normative but arbitrary definition of scientific method which excludes all or most of what has been called science; nor should we simply draw up a list of the uses of the term "science" or "scientific method". Precisely because scientific practice is heterogeneous and we must both save it and be able to judge it, what we need is a redefinition or an *explication* in the technical sense.

Now, historicist methodology (Lakatos' methodology of scientific research programmes, for example, or Laudan's methodology of scientific research traditions) is in the best position to satisfy (a), because its explications are constructed with a view to fitting important episodes in the history of science; it has problems with (b), however, as we have seen in the previous section; and it is in trouble with (c), especially in combining (a) and (c). The reason for this is that the variety of the history of science is so great that the larger the chunck of history we take into consideration, the vaguer the epistemic model we draw from it turns out to be. The reverse is also true: the more accurate and detailed our epistemic model, the narrower the history of science it captures. An incompatibility, or, better, something like an "uncertainty principle" seems to link the requisite of historical suitability and analytical accuracy. The precision of the one is gained at the price of the vagueness of the other. This is why historicist methodology so often makes use of reconstructionist *hubris* to the real history of science and is forced to put it in the "footnotes".

Can an axiological vindication of method do better? Let us first give a look at its procedure.

Axiological vindication treats scientific method as a set of hypothetical imperatives. This is the approach which has been advocated by K. Popper and J. Watkins,[19] but, as we shall see, its final upshot produces a picture of

---

[18] See Popper's well-known passage in which he says that it is "from the methodological decisions which depend upon the definition of science that the scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavours". K. Popper, *The Logic of Scientific Discovery*, Hutchinson, London 1959, p. 55.

[19] See K. Popper, "Replies to My Critics", in *The Philosophy of Karl Popper*, edited by P. A. Schilpp, Open Court, La Salle, Ill. 1974, p. 1036. For Watkins' view of methodologies and their evaluation, see J. Watkins, "The Popperian Approach to Scientific Knowledge", in G. Radnitzky and G. Andersson (ed.), *Progress and Rationality in Science*, Reidel, Dordrecht — Boston 1978, pp. 23—43; and *Science and Scepticism*, Princeton University Press, Princeton, N. J. 1984, Chapter 4.1 and 4.2.

the scientific enterprise which is far weaker than Popper seems willing to accept or at least than he was originally searching for.[20] Under this construal, methodological rules logically stem from premises containing value judgements and descriptive statements. For instance, the rule of submitting hypotheses to severe attempts at falsification may be introduced, in a conditional form, as the conclusion of an argument like:

It is rational to accept true theories,

Submitting hypotheses to severe attempts at falsification is a way of obtaining true theories

---

If you want to be rational, submit hypotheses to severe attempts at falsification.

in which the first premise is a value judgement that states that something is a value or an end of science, and the second premise is a descriptive statement specifying a mean to such an end.

Two moves are needed, then, in this justificatory procedure, that is (1) choosing an end, and (2) determining the means to that end. But here two historicist objections immediately arise tending to prove that even axiological vindication implies that history must be resorted to. First, it could be objected that, since a means-ends link is synthetic, it is only ascertainable through empirical inquiry, whether it be historical or natural.[21] Second, it could be objected that, if the choice of an end for science is not to be arbitrary, it must save past or present scientific practice, at least partly. Let us suppose someone proposes as the goal of science to amuse people. What might be retorted — the objection says — if not showing that, as a matter of fact, scientists have pursued and are still pursuing different goals?

These are serious objections, but although they convincingly show that an examination of current or past scientific activity cannot be dispensed with, they do not prove that such an examination is a test for methodology. On the contrary, we can show that the history or current practice of scientific activity has no probatory role.

Let us begin with the second objection. There are certain minimum consistency requirements that the choice of a goal for science must satisfy. For example, the goal must (i) be logically coherent; (ii) be compatible with other explicitly chosen or implicitly practiced goals, (iii) be epistemically feasible. But

---

[20] See Popper's original intention: "the first task of the logic of knowledge is to put forward a concept of empirical science, in order to make linguistic usage, now somewhat uncertain, as definite as possible, and in order to draw a clear line of demarcation between science and metaphysical ideas". *The Logic of Scientific Discovery*, p. 39. In the Fifties Popper gave up this task and realistically admitted that, in the original formulation, it is impossible to achieve.

[21] This objection has been raised by J. Passmore; see his "The Relevance of History to the Philosophy of Science", in N. Rescher (ed.), *Scientific Explanation and Understanding*, University Press of America, Lanham 1983, p 95. See also L. Laudan, *Science and Values, op. cit.*, p 40, where methodological rules are said to be "empirically testable relations between ends and means".

since these requirements can be defended through general rationality considerations, and since these considerations, precisely because they are general, are too weak to define the range of the admissible goals of science, the problem crops up again as to whether history should not be resorted to in order to limit this range. My own view is that the history of science is not decisive even in this case.

To support this view, I shall first introduce a distinction that I regard as essential. Science has, or may have, several goals, but a fundamental goal exists with respect to which all others, whether they be epistemic or otherwise, are subordinated. I propose to call this fundamental goal the *constitutive goal* and the subordinated ones *regulative goals*, and I maintain that the constitutive goal of science is agreement between cognitive claims and facts. "Subtract accuracy of fit to nature" — Kuhn writes concerning his requirements for a good theory — "and the enterprise may not resemble science at all, bur perhaps instead philosophy". [22]

This remark suggests that the constitutive goal of science does not depend upon an empirical (or historical) argument, it does not recommend itself by the fact that it saves a lot of history or of factual research. Rather, it is proved by a transcendental argument; the search for agreement between cognitive claims and experience is not just a desideratum like any other, or one of the many "themata" in Holton's sense; it is a necessary condition of the very possibility of the scientific enterprise, the form of scientific knowledge, as distinct from philosophy, myth, religion, etc.

Many authors hold the view that the constitutive goal of science is, or is linked to, truth. The question depends on what position one is ready to take in a wide spectrum of epistemological views: on one side of the spectrum, the realist will claim that a theory whose observational consequences have been verified is true or near to truth; on the opposite side, the instrumentalist will deny that the truth of the consequences ever transfers to the theory from which they have been derived; in intermediate positions, others will consider the truth of the consequences only as a hint or a symptom of verisimilitude; and so on.

Although an avalanche of serious objections has recently descended upon realism and although instrumentalism has his own drawbacks, no compelling arguments, either philosophical or historical, may prescribe what view we should assume. But whatever view will be taken, everybody will admit that agreement of cognitive claims with facts is the essential aim of science: this value will be obviously advocated by the realists, for those who ascribe to science the aim of reaching the truth have no better way to pursue their aim than putting forward and accepting theories more and more fitting the known facts; at least implicitly, the same value will be followed also by the instrumentalists, for those who attribute to science the aim of providing instruments for predictions or problem solutions have no other way of

---

[22] T. Kuhn, *The Essential Tension*, The University of Chicago Press, Chicago 1977, p. 331.

distinguishing between good and bad instruments than that of resorting to the agreement with facts of the predictions or the solutions obtained by using them.

It must be conceded, however, that the constitutive value of agreement with facts is not enough to characterize the aim of science; not only it is vague, but in practice it is also combined with one or more regulative values, such as depth, simplicity, precision, unity, and so forth. Will it not, then, be the task of the history of science — the historicist objection comes in again — to tell us which is the best combination to pursue? The history of science may guide us, but only in the same way in which the objects on show in a window, or bought by previous customers, are a guide for a purchaser. In the well-filled but also messy window of the history of science, we can only find suggestions or instantiations, not justifications. Here again, we cannot say that such and such a combination of values is better than others because it saves the goals pursued by certain great scientists $A$, $B$, $C$ ...; but the other way round: if such a combination is worth pursuing, then what $A$, $B$, $C$ ... did was good science. The choice of the best combination is a free one: it must be argued, of course, but in this argumentation the history of science plays, at the very most, a suggestive, not a probatory role.

There remains the question of determining the means. Does not such determination depend upon a factual inquiry? In my view, not even in this case can the history of science be used as a test of methodology.

Let us suppose that $G$ is a certain combination of epistemic goals which include agreement with facts as the constitutive value. Then, *a priori*, many rules are excluded by $G$, but there is a set $M$ of methodological rules ($m_1 \ldots m_n$) that can be used to get $G$. This set contains a continuum or, at least, an indefinite number of rules none of which in particular can be said to follow analyticaly from $G$. By way of example, let us consider the three following rules of elimination:

$m_1$ = Reject that theory which is falsified by fortified observational reports (Popper's rule);

$m_2$ = Reject that series of theories which proves to be regressive (Lakatos' rule);

$m_3$ = Reject that theory which is ineffective (Laudan's rule).

These rules belong to the same set $M$; all three prescribe that cognitive claims running into serious empirical difficulties are to be rejected. The difference lies in the kind of difficulties that are to be considered, in the weight to be attributed to them, and in the times of elimination of the claim when these difficulties have been discovered. $m_1$ is more sensitive or less resistant to contrary evidence than $m_2$ or $m_3$; other rules of the continuum are sensitive to still different degrees.

How can we select a rule of the continuum? Hypothetico-deductivist historicist methodology proposes to select that rule which best passes the tests of the history of science. Let us then suppose that from a sample of history

it results that in many (most, all) cases $G$ has been reached by employing $m_1$. The examination of such a sample is anything but simple, for in practice it may be a very complicated question to ascertain what goal has been reached with what methods. Let us grant, however, that this question may be solved. Are we allowed to conclude that $m_1$ is the best method to get $G$, or that it is better than its rivals $m_2 \ldots m_n$? This view faces at least two main problems.

The first problem concerns the selection of the sample of history, especially the temporal limits which should be taken into consideration. These limits may radically alter the situation, for different temporal limits may support different methods. For example, let us suppose $G$ = getting accurate theories; $m_1$ = do not introduce *ad hoc* hypotheses; $m_2$ = do not introduce *ad hoc* hypotheses unless they are independently testable. It may be the case that, in the short run, $m_2$ leads to $G$ more frequently than $m_1$; but in the long run $m_1$ may turn out to be better than $m_2$. Are there any rules to prescribe how long should the sample be? Obviously, there are not; but if this is so, the history of science does not provide us with the desired tests. We cannot draw univocal conclusions from it, and in any case conclusion we may draw depend to a great extent upon the pragmatic factors that enter into the choice of the sample and into the determination of its width.

The second problem concerns the assumptions we have to make about the universe. We certainly need an assumption of uniformity. But uniformity is not enough. An inference such as "$m_1$ has led to $G$ in the past with such a rate of success; nature is uniform $\therefore$ $m_1$ will lead to $G$ in the future" is not reliable unless we specify a degree of uniformity. But, as Kant clearly saw in his reply to Hume, although we possess a good argument for uniformity—namely, the transcendental argument according to which knowledge would not be possible if uniformity were not assumed—we do not possess any compelling arguments, either transcendental or empirical, to establish a particular degree of uniformity. [23] That means that even a high frequency of cases in which $G$ has been reached by employing $m_1$ leaves us free to employ $m_2$ in the next occasion. All depends on how much we are willing to be dragged by the "inductive inertia", that is on the particular value we assign to this force. Since the determination of such a value largely depends upon pragmatic factors, the history of science does not offer, by itself, univocal suggestions, let alone crucial tests.

True, we learn from experience; but we can learn in many ways. If we want to reach an aim, it is generally rational to make use of the method which in the past proved to be the most efficient for that aim, but sometimes it may turn out

---

[23] See I. Kant, *Critique of Pure Reason*, trans. N. Kemp Swith; McMillan, London 1978, A 654 = B 682: "homogeneity is necessarily presupposed in the manifold of possible experience (although we are not in a position to determine in *a priori* fashion its degree), for in the absence of homogeneity, no empirical concepts, and therefore no experience, would be possible". I examined Kant's view and proposed a transcendental justification of induction in my *Hume, Kant e l'induzione*, Il Mulino, Bologna 1982.

to be rational to give up the received methods. In technology and in practical life we often change the rules and we get better artifacts; in science the same may happen: had Galileo followed the most efficient rules of his time, as assessed by the rate of their past success, we would not have modern empirical science.

Choosing a method is thus a complicated question. Logical considerations (regarding the consistency of the aims we want to pursue), epistemological conceptions (regarding what scientific knowledge is and should be), metaphysical views (relative to the world and its order), pragmatical calculations (in regard to the desirable and hoped results), as well as historical information (relative to the different kinds of science that have been reached in the past and to the rules that have been followed) may motivate our choices but cannot offer univocal indications or tests. The spectrum of the possible rules is doomed to remain wide and the degree of our freedom high.

We get similar conclusions if we adopt a forward-looking hypothetico-deductivist test of methods instead of a backward-looking one. This view regards methods as heuristics and suggests that we check them on the basis of their consequences. As H. Sarkar writes, "if a method frequently gave the wrong heuristic advice, such that the theories a scientist worked with led to dead-ends, one would be justified in saying that the method is unacceptable".[24] But here two crucial questions crop up. How frequently does method have to give wrong advice to be considered unacceptable? And what does "wrong" means in this context? Let us suppose a scientist, or a group of scientists, aims to realize goal $G_1$ working with method $m_1$; and let us suppose that, by so doing, they get scientific results that are more consistent with $G_1$ than with $G_2$. Should they conclude that $m_1$ is wrong and abandon it? Why? Simply because the results they got are different than those they aimed at or than those they had previously obtained? Was then Galileo's method wrong? Moreover, within what temporal limits should we carry on experiments with methods? Thus, forward-looking methodology reproduces the same drawbacks as the backward-looking and the inductivist ones.

Summing up, an axiological construal of method puts the emphasis on the aims we want to realize and makes the rules instrumental to these aims; it stresses the wide freedom we have either in choosing the aims or in determining the best means to those aims; it also makes room for history, but it denies it plays a testing role. An examination of the history of science, or of actual scientific practice, may motivate our choices, for it makes us acquainted with examples of scientific behaviour, but it cannot justify them, for it offers neither warrants or univocal indications. We can learn from history but in the same way in which we learn from experience. And if experience is the name we put

---

[24] H. Sarkar, *A Theory of Method*, University of California Press, Berkeley 1983, p. 153. The use of method as heuristics has been suggested to me also by R. Burian (personal communication).

on our mistakes, historical experience is the name with which we christen our consolations. Narcissus is not wrong in wishing to be consoled; he is wrong in claiming he is the only one that has been consoled.

## 5. FROM DEMONSTRATION TO ARGUMENTATION

We must now turn to the requisites we imposed on any explication of method. We demanded (a) empirical adequacy, (b) normative strength and heuristic power; (c) precision; but what we can get is much less. Under an axiological construal of rules, we can satisfy (a); if we combine this construal with an examination of the history of science and a disposition to learn from it, we can also satisfy (b), although, as we have seen, this demand is not very important, for a method that has scarcely or never been used may lead us to new and better results. But what about (c)?

The "uncertainty principle" still holds good and imposes a limit to the product (a) (c); this product cannot be lower than a certain degree: what we gain in adequacy we lose in precision. It is easy to show that a methodological rule (such as "Reject those hypotheses that are falsified by contrary evidence") which fits accurately, say, Galileo's strategy in one occasion, is not accurate in others; whereas a rule that is adequate to all or most occasions (such as "Reject those hypotheses that run into a lot of anomalies") turns out to exhibit relevant margins of vagueness. In addition to that, there are other reasons which prove that a precise, clear-cut method or logic of discovery cannot be obtained.

In the first place, the constitutive goal of science is indeterminate. We do not possess a general criterion of agreement with facts. To a great extent this notion looks like an empty container that may be filled with many different kinds of things.

In the second place, the constitutive goal combines with several regulative goals. For instance, we pursue agreement of facts plus depth, plus simplicity, plus unity of principles, and so on. And not even these notions can be explicated univocally; they remain largely opaque to our most sophisticated analytical tools.

Thirdly, the means are underdetermined by the ends. More than one methodological rule may be used to achieve the same combination of ends.

Should then we conclude that there are no methods in science? Certainly, we should conclude that the religion of science as demonstration can no longer be maintained, for the Pope of that religion — His Majesty the Scientific Method — has been dethroned for several good reasons. But the fact that we cannot get both adequate and precise rule does not imply that scientific activity has no rules or constraints.

The axiological construal put us on the right track. Kant divided hypothetical imperatives into two classes, technical and pragmatical. The former stem from a problematical end, the latter from an end shared by all human beings, that is, happiness. Now, in the realm of science the idea of

agreement with facts equals Kant's idea of happiness for the sphere of practical behaviour; correspondingly, as happiness can only be reached following what, due to their lack of precision and low degree of injunctive force, Kant called "counsels of prudence" as opposed to "rules of skill", so agreement between cognitive claims with facts can only be obtained (if it can be) by respecting certain suggestions or advice as opposed to strict rules or norms. Strict and clear-cut rules are out of place in the methodology of science. They have to be "relaxed" [25] for, as the axiological vindication emphasizes, not only do they constitute an open class, but they also have an "open texture". [26]

Does this relaxation and open-texture, together with the massive dose of personal, social, cultural and historical ingredients they introduce, mean that science is irrational or close to myth? Certainly, it means that the old ideal of rationality, secured by „certain and easy rules" is untenable. But the failure of that ideal leaves us free to browse new pastures.

The religion of science as demonstration rested, among the others, upon a dogmatic rationalistic presupposition, namely, that these cognitive claims that do not satisfy the scientific method are to be considered irrational (or metaphysical or meaningless). Curiously enough (but not too curiously, if we consider the subtle, unconscious force of persuasion that always descends from a long-practiced religion), the new philosophy of science is a victim of the same dogma and is guilty of the same rationalistic *non sequitur*. According to Descartes, if science had no "certain and easy rules" it would precipitate into the darkness of error and inconclusive disputes. According to Feyerabend, since science has no clear-cut, invariant methods it is close to myth. For Descartes and the methodological tradition stemming from him, what remains outside method may only be irrational passions; for Feyerabend "what remains are aesthetic judgements, judgements of taste, metaphysical prejudices, religious desires, in short what remains are our subjective wishes". [27]

Frightened by these consequences, the historicist methodology of science has tried to provide a remedy for them by "sophisticating" the rules, in order to concede something to the disbelievers without losing the essential of the old religion. If the arguments produced in this paper prove to be correct, that move leads to a dead-end. But if the paradise of science as demonstration is irremediably lost, we are not necessarily condemned to the hell of science as propaganda; the republic of science as argumentation looks like a place more interesting to live and more promising to explore. [28]

---

[25] The notion of "relaxed methodology" has been introduced by C. Hempel in "Valuation and Objectivity in Science", in *Physics, Philosophy and Psychoanalysis, op. cit.*

[26] I construed methodological rules as "open textured" counsels in my *Apologia del metodo*, Laterza, Rome 1982. E. McMullin speaks of the "open-texture" nature of the basic scientific goals in his "The Ambiguity of Historicism", *Current Research in Philosophy of Science*, Philosophy of Science Association, East Lansing 1979, p. 75.

[27] P. Feyerabend, *Against Method*, New Left Books, London 1975, p. 285.

[28] The role of argumentation in science, as well as an outline of a logic of argumentation, is developed, especially with reference to Galileo's *Dialogue*, in M. Finocchiaro, *Galileo and the Art of Reasoning*, Reidel, Dordrecht — Boston 1980. See also my *Apologia del metodo, op. cit.*