

History of Science and Its Rational Reconstructions

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HISTORY OF SCIENCE AND ITS RATIONAL  
RECONSTRUCTIONS\*

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INTRODUCTION

“Philosophy of science without history of science is empty; history of science without philosophy of science is blind”. Taking its cue from this paraphrase of Kant’s famous dictum, this paper intends to explain *how* the historiography of science should learn from the philosophy of science and *vice versa*. It will be argued that (a) philosophy of science provides normative methodologies in terms of which the historian reconstructs ‘internal history’ and thereby provides a rational explanation of the growth of objective knowledge; (b) two competing methodologies can be evaluated with the help of (normatively interpreted) history; (c) any rational reconstruction of history needs to be supplemented by an empirical (socio-psychological) ‘external history’.

The vital demarcation between normative-internal and empirical-external is different for each methodology. Jointly, internal and external historiographical theories determine to a very large extent the choice of

\* The notes are to be found on pp. 122–34. It is to be regretted that they could not be printed at the foot of each page, because they form an integral part of the paper (Ed.).

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problems for the historian. But some of external history's most crucial problems can be formulated only in terms of one's methodology; thus internal history, so defined, is primary, and external history only secondary. Indeed, in view of the autonomy of internal (but not of external) history, external history is irrelevant for the understanding of science.<sup>1</sup>

#### I. RIVAL METHODOLOGIES OF SCIENCE; RATIONAL RECONSTRUCTIONS AS GUIDES TO HISTORY

There are several methodologies afloat in contemporary philosophy of science; but they are all very different from what used to be understood by 'methodology' in the seventeenth or even eighteenth century. Then it was hoped that methodology would provide scientists with a mechanical book of rules for solving problems. This hope has now been given up: modern methodologies or 'logics of discovery' consist merely of a set of (possibly not even tightly knit, let alone mechanical) rules for the *appraisal* of ready, articulated theories.<sup>2</sup> Often these rules, or systems of appraisal, also serve as 'theories of scientific rationality', 'demarcation criteria' or 'definitions of science'.<sup>3</sup> Outside the legislative domain of these normative rules there is, of course, an empirical psychology and sociology of discovery.

I shall now sketch four different 'logics of discovery'. Each will be characterised by rules governing the (scientific) *acceptance* and *rejection* of theories or research programmes.<sup>4</sup> These rules have a double function. First, they function as *a code of scientific honesty* whose violation is intolerable; secondly, as hard cores of (*normative*) *historiographical research programmes*. It is their second function on which I should like to concentrate.

##### A. *Inductivism*

One of the most influential methodologies of science has been inductivism. According to inductivism only those propositions can be accepted into the body of science which either describe hard facts or are infallible inductive generalisations from them.<sup>5</sup> When the inductivist *accepts* a scientific proposition, he accepts it as provenly true; he *rejects* it if it is not. His scientific rigour is strict: a proposition must be either proven from facts, or – deductively or inductively – derived from other propositions already proven.

Each methodology has its specific epistemological and logical problems. For example, inductivism has to establish with certainty the truth of 'factual' ('basic') propositions and the validity of inductive inferences. Some philosophers get so preoccupied with their epistemological and logical problems that they never get to the point of becoming interested in actual history; if actual history does not fit their standards they may even have the temerity to propose that we start the whole business of science anew. Some others take some crude solution of these logical and epistemological problems for granted and devote themselves to a rational reconstruction of history without being aware of the logico-epistemological weakness (or, even, untenability) of their methodology.<sup>6</sup>

Inductivist criticism is primarily sceptical: it consists in showing that a proposition is unproven, that is, pseudoscientific, rather than in showing that it is false.<sup>7</sup> When the inductivist historian writes the *prehistory* of a scientific discipline, he may draw heavily upon such criticisms. And he often explains the early dark age – when people were engrossed by 'unproven ideas' – with the help of some 'external', explanation, like the socio-psychological theory of the retarding influence of the Catholic Church.

The inductivist historian recognizes only two sorts of *genuine scientific discoveries*: *hard factual propositions* and inductive *generalisations*. These and only these constitute the backbone of his *internal history*. When writing history, he looks out for them – finding them is quite a problem. Only when he finds them, can he start the construction of his beautiful pyramids. Revolutions consist in unmasking [irrational] errors which then are exiled from the history of science into the history of pseudoscience, into the history of mere beliefs: genuine scientific progress starts with the latest scientific revolution in any given field.

Each internal historiography has its characteristic victorious paradigms.<sup>8</sup> The main paradigms of inductivist historiography were Kepler's generalisations from Tycho Brahe's careful observations; Newton's discovery of his law of gravitation by, in turn, inductively generalising Kepler's 'phenomena' of planetary motion; and Ampère's discovery of his law of electrodynamics by inductively generalising his observations of electric currents. Modern chemistry too is taken by some inductivists as having really started with Lavoisier's experiments and his 'true explanations' of them.

But the inductivist historian cannot offer a *rational* 'internal' explanation for *why* certain facts rather than others were selected in the first instance. For him this is a *non-rational, empirical, external* problem. Inductivism as an 'internal' theory of rationality is compatible with many different supplementary empirical or external theories of problem-choice. It is, for instance, compatible with the vulgar-Marxist view that problem-choice is determined by social needs;<sup>9</sup> indeed, some vulgar-Marxists identify major phases in history of science with the major phases of economic development.<sup>10</sup> But choice of facts need not be determined by social factors; it may be determined by extra-scientific intellectual influences. And inductivism is equally compatible with the 'external' theory that the choice of problems is primarily determined by inborn, or by arbitrarily chosen (or traditional) theoretical (or 'metaphysical') frameworks.

There is a radical brand of inductivism which condemns all external influences, whether intellectual, psychological or sociological, as creating impermissible bias: radical inductivists allow only a [random] selection by the empty mind. Radical inductivism is, in turn, a special kind of *radical internalism*. According to the latter once one establishes the existence of some external influence on the acceptance of a scientific theory (or factual proposition) one must withdraw one's acceptance: proof of external influence means invalidation:<sup>11</sup> but since external influences always exist, radical internalism is utopian, and, as a theory of rationality, self-destructive.<sup>12</sup>

When the radical inductivist historian faces the problem of why some great scientists thought highly of metaphysics and, indeed, why they thought that their discoveries were great for reasons which, in the light of inductivism, look very odd, he will refer these problems of 'false consciousness' to psychopathology, that is, to external history.

### B. *Conventionalism*

Conventionalism allows for the building of any system of pigeon holes which organises facts into some coherent whole. The conventionalist decides to keep the centre of such a pigeonhole system intact as long as possible: when difficulties arise through an invasion of anomalies, he only changes and complicates the peripheral arrangements. But the conventionalist does not regard any pigeonhole system as provenly true,

but only as 'true by convention' (or possibly even as neither true nor false). In *revolutionary* brands of conventionalism one does not have to adhere forever to a given pigeonhole system: one may abandon it if it becomes unbearably clumsy and if a simpler one is offered to replace it.<sup>13</sup> This version of conventionalism is epistemologically, and especially logically, much simpler than inductivism: it is in no need of valid inductive inferences. Genuine *progress* of science is cumulative and takes place on the ground level of 'proven' facts;<sup>14</sup> the *changes* on the theoretical level are merely instrumental. Theoretical 'progress' is only in convenience ('simplicity'), and not in truth-content.<sup>15</sup> One may, of course, introduce revolutionary conventionalism also at the level of 'factual' propositions, in which case one would accept 'factual' propositions by decision rather than by experimental 'proofs'. But then, if the conventionalist is to retain the idea that the growth of 'factual' science has anything to do with objective, factual truth, he must devise some metaphysical principle which he then has to superimpose on his rules for the game of science.<sup>16</sup> If he does not, he cannot escape scepticism or, at least, some radical form of instrumentalism.

(It is important to clarify the *relation between conventionalism and instrumentalism*. Conventionalism rests on the recognition that false assumptions may have true consequences; therefore false theories may have great predictive power. Conventionalists had to face the problem of comparing rival false theories. Most of them conflated truth with its signs and found themselves holding some version of the pragmatic theory of truth. It was Popper's theory of truth-content, verisimilitude and corroboration which finally laid down the basis of a philosophically flawless version of conventionalism. On the other hand some conventionalists did not have sufficient logical education to realise that some propositions may be true whilst being unproven; and others false whilst having true consequences, and also some which are both false and approximately true. These people opted for 'instrumentalism': they came to regard theories as neither true nor false but merely as 'instruments' for prediction. Conventionalism, as here defined, is a philosophically sound position; instrumentalism is a degenerate version of it, based on a mere philosophical muddle caused by lack of elementary logical competence.)

Revolutionary conventionalism was born as the Bergsonian's philosophy of science: free will and creativity were the slogans. The code of

scientific honour of the conventionalist is less rigorous than that of the inductivist: it puts no ban on unproven speculation, and allows a pigeonhole system to be built around *any* fancy idea. Moreover, conventionalism does not brand discarded systems as unscientific: the conventionalist sees much more of the actual history of science as rational ('internal') than does the inductivist.

For the conventionalist historian, major discoveries are primarily inventions of new and simpler pigeonhole systems. Therefore he constantly compares for simplicity: the complications of pigeonhole systems and their revolutionary replacement by simpler ones constitute the backbone of his internal history.

The paradigmatic case of a scientific revolution for the conventionalist has been the Copernican revolution.<sup>17</sup> Efforts have been made to show that Lavoisier's and Einstein's revolutions too were replacements of clumsy theories by simple ones.

Conventionalist historiography cannot offer a *rational* explanation of why certain facts were selected in the first instance or of why certain particular pigeonhole systems were tried rather than others at a stage when their relative merits were yet unclear. Thus conventionalism, like inductivism, is compatible with various supplementary empirical-'externalist' programmes.

Finally, the conventionalist historian, like his inductivist colleague, frequently encounters the problem of 'false consciousness'. According to conventionalism, for example, it is a 'matter of fact' that great scientists arrive at their theories by flights of their imaginations. Why then do they often claim that they derived their theories from facts? The conventionalist's rational reconstruction often differs from the great scientists' own reconstruction – the conventionalist historian relegates these problems of false consciousness to the externalist.<sup>18</sup>

### C. *Methodological Falsificationism*

Contemporary falsificationism arose as a logico-epistemological criticism of inductivism and of Duhemian conventionalism. Inductivism was criticised on the grounds that its two basic assumptions, namely, that factual propositions can be 'derived' from facts and that there can be valid inductive (content-increasing) inferences, are themselves unproven and even demonstrably false. Duhem was criticised on the grounds that

comparison of intuitive simplicity can only be a matter for subjective taste and that it is so ambiguous that no hard-hitting criticism can be based on it. Popper, in his *Logik der Forschung*, proposed a new ‘falsificationist’ methodology.<sup>19</sup> This methodology is another brand of revolutionary conventionalism: the main difference is that it allows factual, spatio-temporally singular ‘basic statements’, rather than spatio-temporally universal theories, to be accepted by convention. In the code of honour of the falsificationist a theory is scientific only if it can be *made* to conflict with a basic statement; and a theory must be eliminated if it conflicts with an accepted basic statement. Popper also indicated a further condition that a theory must satisfy in order to qualify as scientific: it must predict facts which are *novel*, that is, unexpected in the light of previous knowledge. Thus it is against Popper’s code of scientific honour to propose unfalsifiable theories or ‘ad hoc’ hypotheses (which imply no *novel* empirical predictions) – just as it is against the [classical] inductivist code of scientific honour to propose unproven ones.

The great attraction of Popperian methodology lies in its clarity and force. Popper’s deductive model of scientific criticism contains empirically falsifiable spatio-temporally universal propositions, initial conditions and their consequences. The weapon of criticism is the *modus tollens*: neither inductive logic nor intuitive simplicity complicate the picture.<sup>20</sup>

(Falsificationism, though logically impeccable, has epistemological difficulties of its own. In its ‘dogmatic’ proto-version it assumes the provability of propositions from facts and thus the disprovability of theories – a false assumption.<sup>21</sup> In its Popperian ‘conventionalist’ version it needs some (extra-methodological) ‘inductive principle’ to lend epistemological weight to its decisions to accept ‘basic’ statements, and in general to connect its rules of the scientific game with verisimilitude.<sup>22</sup>)

The Popperian historian looks for great, ‘bold’, falsifiable theories and for great negative crucial experiments. These form the skeleton of his rational reconstruction. The Popperians’ favourite paradigms of great falsifiable theories are Newton’s and Maxwell’s theories, the radiation formulas of Rayleigh, Jeans and Wien, and the Einsteinian revolution; their favourite paradigms for crucial experiments are the Michelson-Morley experiment, Eddington’s eclipse experiment, and the experiments of Lummer and Pringsheim. It was Agassi who tried to turn this naive falsificationism into a systematic historiographical research programme.<sup>23</sup>

In particular he predicted (or ‘postdicted’, if you wish) that behind each great experimental discovery lies a theory which the discovery contradicted; the importance of a factual discovery is to be measured by the importance of the theory refuted by it. Agassi seems to accept at face value the value judgments of the scientific community concerning the importance of factual discoveries like Galvani’s, Oersted’s, Priestley’s, Roentgen’s and Hertz’s; but he denies the ‘myth’ that they were chance discoveries (as the first four were said to be) or confirming instances (as Hertz first thought his discovery was).<sup>24</sup> Thus Agassi arrives at a bold prediction: all these five experiments were successful refutations – in some cases even *planned* refutations – of theories which he proposes to unearth, and, indeed, in most cases, claims to have unearthed.<sup>25</sup>

Popperian internal history, in turn, is readily supplemented by external theories of history. Thus Popper himself explained that [on the positive side] (1) the main *external* stimulus of scientific theories comes from unscientific ‘metaphysics’, and even from myths (this was later beautifully illustrated mainly by Koyré); and that [on the negative side] (2) facts do *not* constitute such external stimulus – factual discoveries belong completely to internal history, emerging as refutations of some scientific theory, so that facts are only noticed if they conflict with some previous expectation. Both theses are cornerstones of Popper’s *psychology* of discovery.<sup>26</sup> Feyerabend developed another interesting *psychological* thesis of Popper, namely, that proliferation of rival theories may – *externally* – speed up *internal* Popperian falsification.<sup>27</sup>

But the external supplementary theories of falsificationism need not be restricted to purely intellectual influences. It has to be emphasized (*pace* Agassi) that falsificationism is no less compatible with a vulgar-Marxist view of what makes science progress than is inductivism. The only difference is that while for the latter Marxism might be invoked to explain the discovery of *facts*, for the former it might be invoked to explain the invention of *scientific theories*; while the choice of facts (that is, for the falsificationist, the choice of ‘potential falsifiers’) is primarily determined internally by the theories.

‘False awareness’ – ‘false’ from the point of view of *his* rationality theory – creates a problem for the falsificationist historian. For instance, why do some scientists believe that crucial experiments are positive and verifying rather than negative and falsifying? It was the falsificationist

Popper who, in order to solve these problems, elaborated better than anybody else before him the cleavage between objective knowledge (in his 'third world') and its distorted reflections in individual minds.<sup>28</sup> Thus he opened up the way for my demarcation between internal and external history.

#### D. *Methodology of Scientific Research Programmes*

According to my methodology the greatest scientific achievements are research programmes which can be evaluated in terms of progressive and degenerating problemshifts; and scientific revolutions consist of one research programme superseding (overtaking in progress) another.<sup>29</sup> This methodology offers a new rational reconstruction of science. It is best presented by contrasting it with falsificationism and conventionalism, from both of which it borrows essential elements.

From conventionalism, this methodology borrows the licence rationally to accept by convention not only spatio-temporally singular 'factual statements' but also spatio-temporally universal theories: indeed, this becomes the most important clue to the continuity of scientific growth.<sup>30</sup> The basic unit of appraisal must be not an isolated theory or conjunction of theories but rather a '*research programme*', with a conventionally accepted (and thus by provisional decision 'irrefutable') '*hard core*' and with a '*positive heuristic*' which defines problems, outlines the construction of a belt of auxiliary hypotheses, foresees anomalies and turns them victoriously into examples, all according to a preconceived plan. The scientist lists anomalies, but as long as his research programme sustains its momentum, he may freely put them aside. *It is primarily the positive heuristic of his programme, not the anomalies, which dictate the choice of his problems.*<sup>31</sup> Only when the driving force of the positive heuristic weakens, may more attention be given to anomalies. The methodology of research programmes can explain in this way *the high degree of autonomy of theoretical science*; the naive falsificationist's disconnected chains of conjectures and refutations cannot. What for Popper, Watkins and Agassi is *external*, influential metaphysics, here turns into the *internal* 'hard core' of a programme.<sup>32</sup>

The methodology of research programmes presents a very different picture of the game of science from the picture of the methodological falsificationist. The best opening gambit is not a falsifiable (and therefore

consistent) hypothesis, but a research programme. Mere ‘falsification’ (in Popper’s sense) must not imply rejection.<sup>33</sup> Mere ‘falsifications’ (that is, anomalies) are to be recorded but need not be acted upon. Popper’s great negative crucial experiments disappear; ‘crucial experiment’ is an honorific title, which may, of course, be conferred on certain anomalies, but only *long after the event*, only when one programme has been defeated by another one. According to Popper a crucial experiment is described by an accepted basic statement which is inconsistent with a theory – according to the methodology of scientific research programmes no accepted basic statement *alone* entitles the scientist to reject a theory. Such a clash may present a problem (major or minor), but in no circumstance a ‘victory’. Nature may shout *no*, but human ingenuity – contrary to Weyl and Popper<sup>34</sup> – may always be able to shout louder. With sufficient resourcefulness and some luck, any theory can be defended ‘progressively’ for a long time, even if it is false. The Popperian pattern of ‘conjectures and refutations’, that is the pattern of trial-by-hypothesis followed by error-shown-by-experiment, is to be abandoned: no experiment is crucial at the time – let alone before – it is performed (except, possibly, psychologically).

It should be pointed out, however, that the methodology of scientific research programmes has more teeth than Duhem’s conventionalism: instead of leaving it to Duhem’s unarticulated common sense<sup>35</sup> to judge when a ‘framework’ is to be abandoned, I inject some hard Popperian elements into the appraisal of whether a programme progresses or degenerates or of whether one is overtaking another. That is, I give criteria of progress and stagnation within a programme and also rules for the ‘elimination’ of whole research programmes. A research programme is said to be *progressing* as long as its theoretical growth anticipates its empirical growth, that is, as long as it keeps predicting novel facts with some success (*‘progressive problemshift’*); it is *stagnating* if its theoretical growth lags behind its empirical growth, that is, as long as it gives only *post-hoc* explanations either of chance discoveries or of facts anticipated by, and discovered in, a rival programme (*‘degenerating problemshift’*).<sup>36</sup> If a research programme progressively explains more than a rival, it ‘supersedes’ it, and the rival can be eliminated (or, if you wish, ‘shelved’).<sup>37</sup>

(*Within* a research programme a theory can only be eliminated by a better theory, that is, by one which has excess empirical content over its predecessors, some of which is subsequently confirmed. And for this

replacement of one theory by a better one, the first theory does not even have to be 'falsified' in Popper's sense of the term. Thus progress is marked by instances verifying excess content rather than by falsifying instances;<sup>38</sup> empirical 'falsification' and actual 'rejection' become independent.<sup>39</sup> Before a theory has been modified we can never know in what way it had been 'refuted', and some of the most interesting modifications are motivated by the 'positive heuristic' of the research programme rather than by anomalies. This difference alone has important consequences and leads to a rational reconstruction of scientific change very different from that of Popper's.<sup>40</sup>)

It is very difficult to decide, especially since one must not demand progress at each single step, when a research programme has degenerated hopelessly or when one of two rival programmes has achieved a decisive advantage over the other. In this methodology, as in Duhem's conventionalism, there can be no instant – let alone mechanical – rationality. *Neither the logician's proof of inconsistency nor the experimental scientist's verdict of anomaly can defeat a research programme in one blow.* One can be 'wise' only after the event.<sup>41</sup>

In this code of scientific honour modesty plays a greater role than in other codes. One *must* realise that one's opponent, even if lagging badly behind, may still stage a comeback. No advantage for one side can ever be regarded as absolutely conclusive. There is never anything inevitable about the triumph of a programme. Also, there is never anything inevitable about its defeat. Thus pigheadedness, like modesty, has more 'rational' scope. *The scores of the rival sides, however, must be recorded<sup>42</sup> and publicly displayed at all times.*

(We should here at least refer to the main epistemological problem of the methodology of scientific research programmes. As it stands, like Popper's methodological falsificationism, it represents a very radical version of conventionalism. One needs to posit some extra-methodological inductive principle to relate – even if tenuously – the scientific gambit of pragmatic acceptances and rejections to verisimilitude.<sup>43</sup> Only such an 'inductive principle' can turn science from a mere game into an epistemologically rational exercise; from a set of lighthearted sceptical gambits pursued for intellectual fun into a – more serious – fallibilist venture of approximating the Truth about the Universe.<sup>44</sup>)

The methodology of scientific research programmes constitutes, like

any other methodology, a historiographical research programme. The historian who accepts this methodology as a guide will look in history for rival research programmes, for progressive and degenerating problem shifts. Where the Duhemian historian sees a revolution merely in simplicity (like that of Copernicus), he will look for a large scale progressive programme overtaking a degenerating one. Where the falsificationist sees a crucial negative experiment, he will 'predict' that there was none, that behind any alleged crucial experiment, behind any alleged single battle between theory and experiment, there is a hidden war of attrition between two research programmes. The outcome of the war is only later linked in the falsificationist reconstruction with some alleged single 'crucial experiment'.

The methodology of research programmes – like any other theory of scientific rationality – must be supplemented by empirical-external history. No rationality theory will ever solve problems like why Mendelian genetics disappeared in Soviet Russia in the 1950's, or why certain schools of research into genetic racial differences or into the economics of foreign aid came into disrepute in the Anglo-Saxon countries in the 1960's. Moreover, to explain different speeds of development of different research programmes we may need to invoke external history. Rational reconstruction of science (in the sense in which I use the term) cannot be comprehensive since human beings are not *completely* rational animals; and even when they act rationally they may have a false theory of their own rational actions.<sup>45</sup>

But the methodology of research programmes draws a demarcation between internal and external history which is markedly different from that drawn by other rationality theories. For instance, what for the falsificationist looks like the (regrettably frequent) phenomenon of irrational adherence to a 'refuted' or to an inconsistent theory and which he therefore relegates to *external* history, may well be explained in terms of my methodology *internally* as a rational defence of a promising research programme. Or, the successful *predictions* of novel facts which constitute serious evidence for a research programme and therefore vital parts of internal history, are irrelevant both for the inductivist and for the falsificationist.<sup>46</sup> For the inductivist and the falsificationist it does not really matter whether the discovery of a fact preceded or followed a theory: only their logical relation is decisive. The 'irrational' impact of the historical coincidence that a theory happened to have *anticipated* a factual discovery,

has no internal significance. Such anticipations constitute 'not proof but [mere] propaganda'.<sup>47</sup> Or again, take Planck's discontent with his own 1900 radiation formula, which he regarded as 'arbitrary'. For the falsificationist the formula was a bold, falsifiable hypothesis and Planck's dislike of it a non-rational mood, explicable only in terms of psychology. However, in my view, Planck's discontent can be explained internally: it was a rational condemnation of an '*ad hoc*' theory.<sup>48</sup> To mention yet another example: for falsificationism irrefutable 'metaphysics' is an external intellectual influence, in my approach it is a vital part of the rational reconstruction of science.

Most historians have hitherto tended to regard the solution of some problems as being the monopoly of externalists. One of these is the problem of the high frequency of *simultaneous discoveries*. For this problem vulgar-Marxists have an easy solution: a discovery is made by many people at the same time, once a social need for it arises.<sup>49</sup> Now what constitutes a 'discovery', and especially a major discovery, depends on one's methodology. For the inductivist, the most important discoveries are factual, and, indeed, such discoveries are frequently made simultaneously. For the falsificationist a *major* discovery consists in the discovery of a theory rather than of a fact. Once a theory is discovered (or rather invented), it becomes public property; and nothing is more obvious than that several people will test it simultaneously and make, simultaneously, (minor) factual discoveries. Also, a published theory is a challenge to devise higher-level, independently testable explanations. For example, given Kepler's ellipses and Galileo's rudimentary dynamics, simultaneous 'discovery' of an inverse square law is not so very surprising: a problem-situation being public, simultaneous solutions can be explained on *purely internal* grounds.<sup>50</sup> The discovery of a new problem however may not be so readily explicable. If one thinks of the history of science as of one of rival research programmes, then most simultaneous discoveries, theoretical or factual, are explained by the fact that research programmes being public property, many people work on them in different corners of the world, possibly not knowing of each other. However, really *novel, major, revolutionary* developments are rarely invented simultaneously. Some alleged simultaneous discoveries of novel programmes are seen as having been simultaneous discoveries only with false hindsight: in fact they are *different* discoveries, merged only later into a single one.<sup>51</sup>

A favourite hunting ground of externalists has been the related problem of why so much importance is attached to – and energy spent on – *priority disputes*. This can be explained only *externally* by the inductivist, naive falsificationist, or the conventionalist; but in the light of the methodology of research programmes some priority disputes are vital *internal* problems, since in this methodology *it becomes all-important for rational appraisal which programme was first in anticipating a novel fact and which fitted in the by now old fact only later*. Some priority disputes can be explained by rational interest and not simply by vanity and greed for fame. It then becomes important that Tychonian theory, for instance, succeeded in explaining – only *post hoc* – the observed phases of, and the distance to, Venus which were originally precisely anticipated by Copernicans;<sup>52</sup> or that Cartesians managed to explain everything that the Newtonians *predicted* – but only *post hoc*. Newtonian optical theory explained *post hoc* many phenomena which were anticipated and first observed by Huyghensians.<sup>53</sup>

All these examples show how the methodology of scientific research programmes turns many problems which had been *external* problems for other historiographies into internal ones. But occasionally the borderline is moved in the opposite direction. For instance there may have been an experiment which was accepted *instantly* – in the absence of a better theory – as a negative crucial experiment. For the falsificationist such acceptance is part of internal history; for me it is not rational and has to be explained in terms of external history.

*Note.* The methodology of research programmes was criticised both by Feyerabend and by Kuhn. According to Kuhn: '[Lakatos] must specify criteria which can be used *at the time* to distinguish a degenerative from a progressive research programme; and so on. Otherwise, *he has told us nothing at all*'.<sup>54</sup> Actually, I *do* specify such criteria. But Kuhn probably meant that '[my] standards have practical force only if they are combined with a *time limit* (what looks like a degenerating problemshift may be the beginning of a much longer period of advance)'.<sup>55</sup> Since I specify no such time limit, Feyerabend concludes that my standards are no more than '*verbal ornaments*'.<sup>56</sup> A related point was made by Musgrave in a letter containing some major constructive criticisms of an earlier draft, in which he demanded that I specify, for instance, at what point dogmatic adherence to a programme ought to be explained 'externally' rather than 'internally'.

Let me try to explain why such objections are beside the point. One may rationally stick to a degenerating programme until it is overtaken by a rival *and even after*. What one must *not* do is to deny its poor public record. Both Feyerabend and Kuhn conflate *methodological* appraisal of a programme with firm *heuristic* advice about what to do.<sup>57</sup> It is perfectly rational to play a risky game; what is irrational is to deceive oneself about the risk.

This does not mean as much licence as might appear for those who stick to a degenerating programme. For they can do this mostly only in private. Editors of scientific journals should refuse to publish their papers which will, in general, contain either solemn reassertions of their position or absorption of counterevidence (or even of rival programmes) by *ad hoc*, linguistic adjustments. Research foundations, too, should refuse money.<sup>58</sup>

These observations also answer Musgrave's objection by separating rational and irrational (or honest and dishonest) adherence to a degenerating programme. They also throw further light on the demarcation between internal and external history. They show that internal history is self-sufficient for the presentation of the history of disembodied science, including degenerating problemshifts. External history explains why some people have false beliefs about scientific progress, and how their scientific activity may be influenced by such beliefs.

### E. *Internal and External History*

Four theories of the rationality of scientific progress – or logics of scientific discovery – have been briefly discussed. It was shown how each of them provides a theoretical framework for the rational reconstruction of the history of science.

Thus the internal history of *inductivists* consists of alleged discoveries of hard facts and of so-called inductive generalisations. The internal history of *conventionalists* consists of factual discoveries and of the erection of pigeonhole systems and their replacement by allegedly simpler ones.<sup>59</sup> The internal history of *falsificationists* dramatises bold conjectures, improvements which are said to be *always* content-increasing and, above all, triumphant 'negative crucial experiments'. The *methodology of research programmes*, finally, emphasizes long-extended theoretical and empirical rivalry of major research programmes, progressive and degenerating problemshifts, and the slowly emerging victory of one programme over the other.

Each rational reconstruction produces some characteristic pattern of rational growth of scientific knowledge. But all of these *normative* reconstructions may have to be supplemented by *empirical* external theories to explain the residual non-rational factors. The history of science is always richer than its rational reconstruction. *But rational reconstruction or internal history is primary, external history only secondary, since the most important problems of external history are defined by internal history.* External history either provides non-rational explanation of the speed, locality, selectiveness etc. of historic events *as interpreted* in terms of internal history; or, when history differs from its rational reconstruction,

it provides an empirical explanation of why it differs. But the *rational* aspect of scientific growth is fully accounted for by one's logic of scientific discovery.

Whatever problem the historian of science wishes to solve, he has first to reconstruct the relevant section of the growth of objective scientific knowledge, that is, the relevant section of 'internal history'. As it has been shown, what constitutes for him internal history, depends on his philosophy, whether he is aware of this fact or not. Most theories of the growth of knowledge are theories of the growth of disembodied knowledge: whether an experiment is crucial or not, whether a hypothesis is highly probable in the light of the available evidence or not, whether a problemshift is progressive or not, is not dependent in the slightest on the scientists' beliefs, personalities or authority. These subjective factors are of no interest for any internal history. For instance, the 'internal historian' records the Proutian programme with its hard core (that atomic weights of pure chemical elements are whole numbers) and its positive heuristic (to overthrow, and replace, the contemporary false observational theories applied in measuring atomic weights). This programme was later carried through.<sup>60</sup> The internal historian will waste little time on Prout's *belief* that if the 'experimental techniques' of *his time* were 'carefully' applied, and the experimental findings properly interpreted, the anomalies would *immediately* be seen as mere illusions. The internal historian will regard this historical fact as a fact in the second world which is only a caricature of its counterpart in the third world.<sup>61</sup> *Why* such caricatures come about is none of his business; he might – in a footnote – pass on the externalist the problem of why certain scientists had 'false beliefs' about what they were doing.<sup>62</sup>

Thus in constructing internal history the historian will be highly selective: he will omit everything that is irrational in the light of his rationality theory. But this normative selection still does not add up to a fully fledged rational reconstruction. For instance, Prout never articulated the 'Proutian programme': the Proutian programme is not Prout's programme. *It is not only the ('internal') success or the ('internal') defeat of a programme which can only be judged with hindsight: it is frequently also its content.* Internal history is not just a *selection* of methodologically interpreted facts: it may be, on occasions, their *radically improved version*. One may illustrate this using the Bohrian programme. Bohr, in 1913, may not have

even thought of the possibility of electron spin. He had more than enough on his hands without the spin. Nevertheless, the historian, describing with hindsight the Bohrian programme, should include electron spin in it, since electron spin fits naturally in the original outline of the programme. Bohr might have referred to it in 1913. Why Bohr did not do so, is an interesting problem which deserves to be indicated in a footnote.<sup>63</sup> (Such problems might then be solved either internally by pointing to rational reasons in the growth of objective, impersonal knowledge; or externally by pointing to psychological causes in the development of Bohr's personal beliefs.)

One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text*, and indicate *in the footnotes* how actual history 'misbehaved' in the light of its rational reconstruction.<sup>64</sup>

Many historians will abhor the idea of *any* rational reconstruction. They will quote Lord Bolingbroke: 'History is philosophy teaching by example'. They will say that before philosophising 'we need a lot more examples'.<sup>65</sup> But such an inductivist theory of historiography is utopian.<sup>66</sup> *History without some theoretical 'bias' is impossible.*<sup>67</sup> Some historians look for the discovery of hard facts, inductive generalisations, others for bold theories and crucial negative experiments, yet others for great simplifications, or for progressive and degenerating problemshifts; all of them have *some* theoretical 'bias'. This bias, of course, may be obscured by an eclectic variation of theories or by theoretical confusion: but neither eclecticism nor confusion amounts to an atheoretical outlook. What a historian regards as an external problem is often an excellent guide to his implicit methodology: some will ask why a 'hard fact' or a 'bold theory' was discovered exactly when and where it actually was discovered; others will ask why a 'degenerating problemshift' could have wide popular acclaim over an incredibly long period or why a 'progressive problemshift' was left 'unreasonably' unacknowledged.<sup>68</sup> Long texts have been devoted to the problem of whether, and if so, why, the emergence of science was a purely European affair; but such an investigation is bound to remain a piece of confused rambling until one clearly defines 'science' according to some normative philosophy of science. One of the most interesting problems of external history is to specify the psychological, and indeed, social conditions which are necessary (but, of course, never sufficient) to

make scientific progress possible; but in the very formulation of this 'external' problem *some* methodological theory, *some* definition of science is bound to enter. History of *science* is a history of events which are selected and interpreted in a normative way.<sup>69</sup> This being so, the hitherto neglected problem of appraising rival logics of scientific discovery and, hence, rival reconstructions of history, acquires paramount importance. I shall now turn to this problem.

## 2. CRITICAL COMPARISON OF METHODOLOGIES: HISTORY AS A TEST OF ITS RATIONAL RECONSTRUCTIONS

Theories of scientific rationality can be classified under two main heads.

(1) *Justificationist methodologies* set very high epistemological standards: for classical justificationists a proposition is 'scientific' only if it is *proven*, for neojustificationists, if it is *probable* (in the sense of the probability calculus) or *corroborated* (in the sense of Popper's third note on corroboration) to a proven degree.<sup>70</sup> Some philosophers of science gave up the idea of proving or of (provably) probabilifying scientific theories but remained dogmatic empiricists: whether inductivists, probabilists, conventionalists or falsificationist, they still stick to the provability of 'factual' propositions. By now, of course, all these different forms of justificationism have crumbled under the weight of *epistemological and logical criticism*.

(2) The only alternatives with which we are left are *pragmatic-conventionalist methodologies*, crowned by some global principle of induction. Conventionalist methodologies first lay down rules about 'acceptance' and 'rejection' of factual and theoretical propositions – without yet laying down rules about proof and disproof, truth and falsehood. We then get *different systems of rules of the scientific game*. The inductivist game would consist of collecting 'acceptable' (not proven) data and drawing from them 'acceptable' (not proven) inductive generalisations. The conventionalist game would consist of collecting 'acceptable' data and ordering them into the simplest possible pigeonhole systems (or devising the simplest possible pigeonhole systems and filling them with acceptable data). Popper specified yet another game as 'scientific'.<sup>71</sup> Even methodologies which have been epistemologically and logically discredited, may go on functioning, in these emasculated versions, as guides for the rational reconstruction of history. But these *scientific games* are without any

genuine epistemological relevance *unless* we superimpose on them some sort of metaphysical (or, if you wish, 'inductive') principle which will say that the game, as specified by the methodology, gives us the best chance of approaching the Truth. Such a principle then turns the pure conventions of the game into fallible conjectures; but without such a principle the scientific game is just like any other game.<sup>72</sup>

It is very difficult to criticise conventionalist methodologies like Duhem's and Popper's. There is no obvious way to criticise either a game or a metaphysical principle of induction. In order to overcome these difficulties I am going to propose a new theory of how to appraise such methodologies of science (the ones, which – at least in the first stage, before the introduction of an inductive principle – are conventionalist). I shall show that methodologies may be criticised without any direct reference to any epistemological (or even logical) theory, and without using directly any logico-epistemological criticism. The basic idea of this criticism is that *all methodologies function as historiographical (or meta-historical) theories (or research programmes) and can be criticised by criticising the rational historical reconstructions to which they lead.*

I shall try to develop this historiographical method of criticism in a dialectical way. I start with a special case: I first 'refute' falsificationism by 'applying' falsificationism (on a normative historiographical meta-level) to itself. Then I shall apply falsificationism also to inductivism and conventionalism, and, indeed, argue that all methodologies are bound to end up 'falsified' with the help of this Pyrrhonian *machine de guerre*. Finally, I shall 'apply' not falsificationism but the methodology of scientific research programmes (again on a normative-historiographical meta-level) to inductivism, conventionalism, falsificationism and to itself, and show that – on this meta-criterion – methodologies can be constructively criticised and compared. This normative-historiographical version of the methodology of scientific research programmes supplies a general theory of how to compare rival logics of discovery in which (in a sense carefully to be specified) *history may be seen as a 'test' of its rational reconstructions.*

#### A. *Falsificationism as a Meta-criterion: History 'falsifies' Falsificationism (and any other Methodology)*

In their purely 'methodological' versions scientific appraisals, as has already been said, are *conventions* and can always be formulated as a

definition of science.<sup>73</sup> How can one criticise such a definition? If one interprets it nominalistically,<sup>74</sup> a definition is a mere abbreviation, a terminological suggestion, a tautology. How can one criticise a tautology? Popper, for one, claims that his definition of science is ‘fruitful’ because ‘a great many points can be clarified and explained with its help’. He quotes Menger: ‘Definitions are dogmas; only the conclusions drawn from them can afford us any new insight’.<sup>75</sup> But how can a definition have explanatory power or afford new insights? Popper’s answer is this: ‘It is only from the consequences of my definition of empirical science, and from the methodological decisions which depend upon this definition, that the scientist will be able to see how far it conforms to his intuitive idea of the goal of his endeavours’.<sup>76</sup>

The answer complies with Popper’s general position that conventions can be criticised by discussing their ‘suitability’ relative to some purpose: ‘As to the suitability of any convention opinions may differ; and a reasonable discussion of these questions is only possible between parties having some purpose in common. The choice of that purpose ... goes beyond rational argument’.<sup>77</sup> Indeed, Popper never offered a theory of rational criticism of consistent conventions. He does not raise, let alone answer, the question: ‘*Under what conditions would you give up your demarcation criterion?*’<sup>78</sup>

But the question can be answered. I give my answer in two stages: I propose first a naive and then a more sophisticated answer. I start by recalling how Popper, according to his own account<sup>78a</sup>, arrived at his criterion. He thought, like the best scientists of his time, that Newton’s theory, although refuted, was a wonderful scientific achievement; that Einstein’s theory was still better; and that astrology, Freudianism and twentieth century Marxism were pseudo-scientific. His problem was to find a definition of science which yielded these ‘*basic judgments*’ concerning particular theories; and he offered a novel solution. Now let us consider the proposal that *a rationality theory – or demarcation criterion – is to be rejected if it is inconsistent with an accepted ‘basic value judgment’ of the scientific elite*. Indeed, this meta-methodological rule (*meta-falsificationism*) would seem to correspond to Popper’s methodological rule (falsificationism) that a scientific theory is to be rejected if it is inconsistent with an (‘empirical’) basic statement unanimously accepted by the scientific community. Popper’s whole methodology rests on the contention

that there exist (relatively) singular statements on whose truth-value scientists can reach unanimous agreement; without such agreement there would be a new Babel and 'the soaring edifice of science would soon lie in ruins'.<sup>79</sup> But even if there were an agreement about 'basic' statements, if there were no agreement about how to appraise scientific achievement relative to this 'empirical basis', would not the soaring edifice of science equally soon lie in ruins? No doubt it would. While there has been little agreement concerning a *universal* criterion of the scientific character of theories, there has been considerable agreement over the last two centuries concerning *single* achievements. While there has been no *general* agreement concerning a theory of scientific rationality, there has been considerable agreement concerning whether a particular single step in the game was scientific or crankish, or whether a particular gambit was played correctly or not. A general definition of science thus must reconstruct the acknowledgedly best gambits as 'scientific': if it fails to do so, it has to be rejected.<sup>80</sup>

Then let us propose tentatively that *if a demarcation criterion is inconsistent with the 'basic' appraisals of the scientific elite, it should be rejected.*

Now *if* we apply this quasi-empirical meta-criterion (which I am going to reject later), Popper's demarcation criterion – that is, Popper's rules of the game of science – has to be rejected.<sup>81</sup>

Popper's basic rule is that the scientist must specify in advance under what experimental conditions he will give up even his most basic assumptions. For instance, he writes, when criticising psychoanalysis: '*Criteria of refutation* have to be laid down beforehand: it must be agreed which observable situations, if actually observed, mean that the theory is refuted. But what kind of clinical responses would refute to the satisfaction of the analyst *not merely a particular analytic diagnosis but psychoanalysis itself?* And have such criteria ever been discussed or agreed upon by analysts?'<sup>82</sup> In the case of psychoanalysis Popper was right: no answer has been forthcoming. Freudians have been nonplussed by Popper's basic challenge concerning scientific honesty. Indeed, they have refused to specify experimental conditions under which they would give up their basic assumptions. For Popper this was the hallmark of their intellectual dishonesty. But what if we put Popper's question to the Newtonian scientist: 'What kind of observation would refute to the satisfaction of the New-

tonian not merely a particular Newtonian explanation but Newtonian dynamics and gravitational theory itself? And have such criteria ever been discussed or agreed upon by Newtonians?' The Newtonian will, alas, scarcely be able to give a positive answer.<sup>83</sup> But then if analysts are to be condemned as dishonest by Popper's standards, Newtonians must also be condemned. Newtonian science, however, in spite of this sort of 'dogmatism', is highly regarded by the greatest scientists, and, indeed, by Popper himself. Newtonian 'dogmatism' then is a 'falsification' of Popper's definition: it defies Popper's rational reconstruction.

Popper may certainly withdraw his celebrated challenge and demand falsifiability – and rejection on falsification – only for systems of theories, including initial conditions and all sorts of auxiliary and observational theories.<sup>84</sup> This is a considerable withdrawal, for it allows the imaginative scientist to save his pet theory by suitable lucky alterations in some odd, obscure corner on the periphery of his theoretical maze. But even Popper's mitigated rule will show up even the most brilliant scientists as irrational dogmatists. For in large research programmes there are always known anomalies: normally the researcher puts them aside and follows the positive heuristic of the programme.<sup>85</sup> In general he rivets his attention on the positive heuristic rather than on the distracting anomalies, and hopes that the 'recalcitrant instances' will be turned into confirming instances as the programme progresses. On Popper's terms the greatest scientists in these situations used forbidden gambits, *ad hoc* stratagems: instead of regarding Mercury's anomalous perihelion as a falsification of the Newtonian theory of our planetary system and thus as a reason for its rejection, most physicists shelved it as a problematic instance to be solved at some later stage – or offered *ad hoc* solutions. This methodological attitude of treating as (mere) *anomalies* what Popper would regard as (dramatic) counterexamples is commonly accepted by the best scientists. Some of the research programmes now held in highest esteem by the scientific community progressed in an ocean of anomalies.<sup>86</sup> That in their choice of problems the greatest scientists 'uncritically' ignore anomalies (and that they isolate them with the help of *ad hoc* stratagems) offers, at least on our meta-criterion, a further falsification of Popper's methodology. He cannot interpret as rational some most important patterns in the growth of science.

Furthermore, for Popper, working on *an inconsistent system* must

invariably be regarded as irrational 'a self-contradictory system must be rejected... [because it] is uninformative... No statement is singled out... since all are derivable'.<sup>87</sup> But some of the greatest scientific research programmes progressed on inconsistent foundations.<sup>88</sup> Indeed in such cases the best scientists' rule is frequently: '*Allez en avant et la foi vous viendra*'. This anti-Popperian methodology secured a breathing space both for the infinitesimal calculus and for naive set theory when they were bedevilled by logical paradoxes.

Indeed, if the game of science had been played according to Popper's rule book, Bohr's 1913 paper would never have been published because it was inconsistently grafted on to Maxwell's theory, and Dirac's delta functions would have been suppressed until Schwartz. All these examples of research based on inconsistent foundations constitute further 'falsifications' of falsificationist methodology.<sup>89</sup>

Thus several of the 'basic' appraisals of the scientific *élite* 'falsify' Popper's definition of science and scientific ethics. The problem then arises, to what extent, given these considerations, can falsificationism function as a guide for the historian of science. The simple answer is, to a very small extent. Popper, the leading falsificationist, never wrote any history of science; possibly because he was too sensitive to the judgment of great scientists to pervert history in a falsificationist vein. One should remember that while in his autobiographical recollections he mentions Newtonian science as the paradigm of scientificity, that is, of falsifiability, in his classical *Logik der Forschung* the falsifiability of Newton's theory is nowhere discussed. The *Logik der Forschung*, on the whole, is dryly abstract and highly ahistorical.<sup>90</sup> Where Popper does venture to remark casually on the falsifiability of major scientific theories, he either plunges into some logical blunder,<sup>91</sup> or distorts history to fit his rationality theory. If a historian's methodology provides a poor rational reconstruction, he may either misread history in such a way that it coincides with his rational reconstruction, or he will find that the history of science is highly irrational. Popper's respect for great science made him choose the first option, while the disrespectful Feyerabend chose the second.<sup>92</sup> Thus Popper, in his historical asides, tends to turn anomalies into 'crucial experiments' and to exaggerate their immediate impact on the history of science. Through his spectacles, great scientists accept refutations readily and this is the primary source of their problems. For instance, in one

place he claims that the Michelson-Morley experiment decisively overthrew classical ether theory; he also exaggerates the role of this experiment in the emergence of Einstein's relativity theory.<sup>93</sup> It takes a naive falsificationist's simplifying spectacles to see, with Popper, Lavoisier's classical experiments as refuting (or as 'tending to refute') the phlogiston theory; or to see the Bohr-Kramers-Slater theory as being knocked out with a single blow from Compton; or to see the parity principle 'rejected' by 'counterexample'.<sup>94</sup>

Furthermore, if Popper wants to reconstruct the provisional acceptance of theories as rational on *his* terms, he is bound to ignore the historical fact that most important theories are born refuted and that some laws are further explained, rather than rejected, in spite of the known counterexamples. He tends to turn a blind eye on all anomalies known before the one which later was enthroned as 'crucial counter-evidence'. For instance, he mistakenly thinks that 'neither Galileo's nor Kepler's theories were refuted before Newton'.<sup>95</sup> The context is significant. Popper holds that the most important pattern of scientific progress is when a crucial experiment leaves one theory *unrefuted* while it refutes a rival one. But, as a matter of fact, in most, if not in all, cases where there are two rival theories, both are known to be simultaneously infected by anomalies. In such situations Popper succumbs to the temptation to simplify the situation into one to which his methodology is applicable.<sup>96</sup>

Falsificationist historiography is then 'falsified'. But if we apply the same meta-falsificationist method to inductivist and conventionalist historiographies, we shall 'falsify' them too.

The best logico-epistemological demolition of inductivism is, of course, Popper's; but even if we assumed that inductivism were philosophically (that is, epistemologically and logically) sound, Duhem's historiographical criticism falsifies it. Duhem took the most celebrated '*successes*' of *inductivist historiography*: Newton's law of gravitation and Ampère's electromagnetic theory. These were said to be two most victorious applications of inductive method. But Duhem (and, following him, Popper and Agassi) showed that they were not. Their analyses illustrate how the inductivist, if he wants to show that the growth of actual science is rational, must falsify actual history out of all recognition.<sup>97</sup> Therefore, if the rationality of science is inductive, actual science is not rational; if it is rational, it is not inductive.<sup>98</sup>

Conventionalism – which, unlike inductivism, is no easy prey to logical or epistemological criticism<sup>99</sup> – can also be historiographically falsified. One can show that the clue to scientific revolutions is not the replacement of cumbersome frameworks by simpler ones.

The Copernican revolution was generally taken to be the *paradigm of conventionalist historiography*, and it is still so regarded in many quarters. For instance Polanyi tells us that Copernicus's 'simpler picture' had 'striking beauty' and '[justly] carried great powers of conviction.'<sup>100</sup> But modern study of primary sources, particularly by Kuhn,<sup>101</sup> has dispelled this myth and presented a clear-cut historiographical refutation of the conventionalist account. It is now agreed that the Copernican system was 'at least as complex as the Ptolemaic'.<sup>102</sup> But if this is so, then, if the acceptance of Copernican theory was rational, it was not for its superlative objective simplicity.<sup>103</sup>

Thus inductivism, falsificationism and conventionalism can be falsified as rational reconstructions of history with the help of the sort of historiographical criticism I have adduced.<sup>104</sup> Historiographical falsification of inductivism, as we have seen, was initiated already by Duhem and continued by Popper and Agassi. Historiographical criticisms of [naive] falsificationism have been offered by Polanyi, Kuhn, Feyerabend and Holton.<sup>105</sup> The most important historiographical criticism of conventionalism is to be found in Kuhn's – already quoted – masterpiece on the Copernican revolution.<sup>106</sup> The upshot of these criticisms is that all these rational reconstructions of history force history of science into the Procrustean bed of their hypocritical morality, thus creating fancy histories, which hinge on mythical 'inductive bases', 'valid inductive generalisations', 'crucial experiments', 'great revolutionary simplifications' etc. But critics of falsificationism and conventionalism drew very different conclusions from the falsification of these methodologies than Duhem, Popper and Agassi did from their own falsification of inductivism. Polanyi (and, seemingly, Holton) concluded that while proper, rational scientific appraisal can be made in *particular* cases, there can be no *general* theory of scientific rationality.<sup>107</sup> *All* methodologies, *all* rational reconstructions can be historiographically 'falsified': science *is* rational, but its rationality cannot be subsumed under the general laws of any methodology.<sup>108</sup> Feyerabend, on the other hand, concluded that not only can there be no general theory of scientific rationality but also that there is no such thing

as scientific rationality.<sup>109</sup> Thus Polanyi swung towards conservative authoritarianism, while Feyerabend swung towards sceptical anarchism. Kuhn came up with a highly original vision of irrationally changing rational authority.<sup>110</sup>

Although, as it transpires from this section, I have high regard for Polanyi's, Feyerabend's and Kuhn's criticisms of extant ('internalist') theories of method, I drew a conclusion completely different from theirs. I decided to look for an improved methodology which offers a better *rational* reconstruction of science.

Feyerabend and Kuhn immediately tried to 'falsify' my improved methodology in turn.<sup>111</sup> I soon had to discover that, at least in the sense described in the present section, my methodology too – and any methodology whatsoever – *can* be 'falsified', for the simple reason that no set of human judgments is completely rational and thus no rational reconstruction can ever coincide with actual history.<sup>112</sup>

This recognition led me to propose a new *constructive* criterion by which methodologies *qua* rational reconstructions of history might be appraised.

#### B. *The Methodology of Historiographical Research Programmes. History – to Varying Degrees – Corroborates Its Rational Reconstructions*

I should like to present my proposal in two stages. First, I shall amend slightly the falsificationist historiographical meta-criterion just discussed, and then replace it altogether with a better one.

First, the slight amendment. If a universal rule clashes with a particular 'normative basic judgment', one should allow the scientific community time to ponder the clash: they may give up their particular judgment and submit to the general rule. 'Second-order' – historiographical – falsifications must not be rushed any more than 'first order' – scientific – ones.<sup>113</sup>

Secondly, since we have abandoned naive falsificationism in *method*, why should we stick to it in *meta-method*? We can easily replace it with a methodology of scientific research programmes of second order, or if you wish, a methodology of historiographical research programmes.

While maintaining that a theory of rationality has to try to organise basic value judgments in universal, coherent frameworks, we do not have to reject such a framework immediately merely because of some anomalies or other inconsistencies. We should, of course, insist that a good

rationality theory must anticipate further basic value judgments unexpected in the light of its predecessors or that it must even lead to the revision of previously held basic value-judgments.<sup>114</sup> We then reject a rationality theory only for a better one, for one which, in this ‘quasi-empirical’ sense, represents a *progressive shift* in the sequence of research programmes of rational reconstructions. Thus this new – more lenient – meta-criterion enables us to compare rival logics of discovery and discern growth in ‘meta-scientific’ – methodological – knowledge.

For instance, Popper’s theory of scientific rationality need not be rejected simply because it is ‘falsified’ by some actual ‘basic judgments’ of leading scientists. Moreover, on our new criterion, Popper’s demarcation criterion clearly represents progress over its justificationist predecessors, and in particular, over inductivism. For, contrary to these predecessors, it rehabilitated the scientific status of falsified theories like phlogiston theory, thus reversing a value judgment which had expelled the latter from the history of science proper into the history of irrational beliefs.<sup>115</sup> Also, it successfully rehabilitated the Bohr-Kramers-Slater theory.<sup>116</sup> In the light of most justificationist theories of rationality the history of science is, at its best, a history of *prescientific* preludes to some *future* history of science.<sup>117</sup> Popper’s methodology enabled the historian to interpret more of the *actual* basic value judgments in the history of science as rational: in *this* normative-historiographical sense Popper’s theory constituted progress. In the light of better rational reconstructions of science one can always reconstruct more of actual great science as rational.<sup>118</sup>

I hope that my modification of Popper’s logic of discovery will be seen, in turn – on the criterion I specified – as yet a further step forward. For it seems to offer a coherent account of *more* old, isolated basic value judgments; moreover, it has led to new and, at least for the justificationist or naive falsificationist, surprising basic value judgments. For instance, according to Popper’s theory, it was irrational to retain and further elaborate Newton’s gravitational theory after the discovery of Mercury’s anomalous perihelion; or again, it was irrational to develop Bohr’s old quantum theory based on inconsistent foundations. From my point of view these were perfectly rational developments: some rearguard actions in the defence of defeated programmes – even after the so-called ‘crucial experiments’ – are perfectly rational. Thus my methodology leads to the

reversal of those historiographical judgments which deleted these rear-guard actions both from inductivist and from falsificationist party histories.<sup>119</sup>

Indeed, this methodology confidently predicts that where the falsificationist sees the instant defeat of a theory through a simple battle with some fact, the historian will detect a complicated war of attrition, starting long before, and ending after, the alleged ‘crucial experiment’; and where the falsificationist sees consistent and unrefuted theories, it predicts the existence of hordes of known anomalies in research programmes progressing on possibly inconsistent foundations.<sup>120</sup> Where the conventionalist sees the clue to the victory of a theory over its predecessor in the former’s intuitive simplicity, this methodology predicts that it will be found that victory was due to empirical degeneration in the old and empirical progress in the new programme.<sup>121</sup> Where Kuhn and Feyerabend see irrational change, I predict that the historian will be able to show that there has been rational change. The methodology of research programmes thus predicts (or, if you wish, ‘postdicts’) novel historical facts, unexpected in the light of extant (internal and external) historiographies and these predictions will, I hope, be corroborated by historical research. If they are, then the methodology of scientific research programmes will itself constitute a progressive problemshift.

*Thus progress in the theory of scientific rationality is marked by discoveries of novel historical facts, by the reconstruction of a growing bulk of value-impregnated history as rational.*<sup>122</sup> In other words, the theory of scientific rationality progresses if it constitutes a ‘progressive’ historiographical research programme. I need not say that no such historiographical research programme can or should explain *all* history of science as rational: even the greatest scientists make false steps and fail in their judgment. Because of this *rational reconstructions remain for ever submerged in an ocean of anomalies. These anomalies will eventually have to be explained either by some better rational reconstruction or by some ‘external’ empirical theory.*

This approach does not advocate a cavalier attitude to the ‘basic normative judgments’ of the scientist. ‘Anomalies’ may be rightly ignored by the internalist *qua* internalist and relegated to external history only as long as the internalist historiographical research programme is *progressing*; or if a supplementary empirical externalist historiographical programme

absorbs them *progressively*. But if in the light of a rational reconstruction the history of science is seen as increasingly irrational *without* a progressive externalist explanation (such as an explanation of the degeneration of science in terms of political or religious terror, or of an antiscientific ideological climate, or of the rise of a new parasitic class of pseudoscientists with vested interests in rapid ‘university expansion’), then historiographical innovation, proliferation of historiographical theories, is vital. Just as scientific progress is possible even if one never gets rid of scientific anomalies, progress in rational historiography is also possible even if one never gets rid of historiographical anomalies. The rationalist historian need not be disturbed by the fact that actual history is more than, and, on occasions, even different from, internal history, and that he may have to relegate the explanation of such anomalies to external history. But this unfalsifiability of internal history does not render it immune to constructive, but only to negative, criticism – just as the unfalsifiability of a scientific research programme does not render it immune to constructive, but only to negative, criticism.

Of course, one can criticise internal history only by making the historian’s (usually latent) methodology explicit, showing how it functions as a historiographical research programme. Historiographical criticism frequently succeeds in destroying much of fashionable externalism. An ‘impressive’, ‘sweeping’, ‘far-reaching’ external explanation is usually the hallmark of a weak methodological substructure; and, in turn, the hallmark of a relatively weak internal history (in terms of which most actual history is either inexplicable or anomalous) is that it leaves too much to be explained by external history. When a better rationality theory is produced, internal history may expand and reclaim ground from external history. The competition, however, is not as open in such cases as when two rival scientific research programmes compete. Externalist historiographical programmes which supplement internal histories based on naive methodologies (whether aware or unaware of the fact) are likely either to degenerate quickly or never even to get off the ground, for the simple reason that they set out to offer psychological or sociological ‘explanations’ of methodologically induced fantasies rather than of (more rationally interpreted) historical facts. Once an externalist account uses, whether consciously or not, a naive methodology (which can so easily creep into its ‘descriptive’ language), it turns into a fairy tale which, for

all its apparent scholarly sophistication, will collapse under historiographical scrutiny.

Agassi already indicated how the poverty of inductivist history opened the door to the wild speculations of vulgar-Marxists.<sup>123</sup> His falsificationist historiography, in turn, flings the door wide open to those trendy ‘sociologists of knowledge’ who try to explain the further (possibly unsuccessful) development of a theory ‘falsified’ by a ‘crucial experiment’ as the manifestation of the irrational, wicked, reactionary resistance by established authority to enlightened revolutionary innovation.<sup>124</sup> But in the light of the methodology of scientific research programmes such rearguard skirmishes are perfectly explicable *internally*: where some externalists see power struggle, sordid personal controversy, the rationalist historian will frequently find rational discussion.<sup>125</sup>

An interesting example of how a poor theory of rationality may impoverish history is the treatment of degenerating problemshifts by historiographical positivists.<sup>126</sup> Let us imagine for instance that in spite of the objectively progressing astronomical research programmes, the astronomers are suddenly all gripped by a feeling of Kuhnian ‘crisis’; and then they all are converted, by an irresistible *Gestalt*-switch, to astrology. I would regard this catastrophe as a horrifying *problem*, to be accounted for by some empirical externalist explanation. But not a Kuhnian. All he sees is a ‘crisis’ followed by a mass conversion effect in the scientific community: an ordinary revolution. Nothing is left as problematic and unexplained.<sup>127</sup> The Kuhnian psychological epiphenomena of ‘crisis’ and ‘conversion’ can accompany either objectively progressive or objectively degenerating changes, either revolutions or counterrevolutions. But this fact falls outside Kuhn’s framework. Such historiographical anomalies cannot be formulated, let alone be progressively absorbed, by his historiographical research programme, in which there is no way of distinguishing between, say, a ‘crisis’ and ‘degenerating problemshift’. But such anomalies might even be predicted by an externalist historiographical theory based on the methodology of scientific research programmes that would specify social conditions under which degenerating research programmes may achieve socio-psychological victory.

### C. *Against Aprioristic and Antitheoretical Approaches to Methodology*

Finally, let us contrast the theory of rationality here discussed with the

strictly aprioristic (or, more precisely, 'Euclidean') and with the anti-theoretical approaches.<sup>128</sup>

'Euclidean' methodologies lay down *a priori general rules* for scientific appraisal. This approach is most powerfully represented today by Popper. In Popper's view there must be the constitutional authority of an *immutable statute law* (laid down in his demarcation criterion) to distinguish between good and bad science.

Some eminent philosophers, however, ridicule the idea of statute law, the possibility of any valid demarcation. According to Oakeshott and Polanyi there must be – and can be – no statute law at all: only case law. They may also argue that even if one mistakenly allowed for statute law, statute law too would need authoritative interpreters. I think that Oakeshott's and Polanyi's position has a great deal of truth in it. After all, one must admit (*pace* Popper) that until now all the 'laws' proposed by the apriorist philosophers of science have turned out to be wrong in the light of the verdicts of the best scientists. Up to the present day it has been the scientific standards, as applied 'instinctively' by the scientific *élite* in *particular* cases, which have constituted the main – although not the exclusive – yardstick of the philosopher's *universal* laws. But if so, methodological progress, at least as far as the most advanced sciences are concerned, still lags behind common scientific wisdom. Is it not then *hubris* to try to impose some *a priori* philosophy of science on the most advanced sciences? Is it not *hubris* to demand that if, say, Newtonian or Einsteinean science turns out to have violated Bacon's, Carnap's or Popper's *apriori* rules of the game, the business of science should be started anew?

I think it is. And, indeed, the methodology of historiographical research programmes implies a pluralistic system of authority, partly because the wisdom of the scientific jury and its case law has not been, and cannot be, fully articulated by the philosopher's statute law, and partly because the philosopher's statute law may occasionally be right when the scientists' judgment fails. I disagree, therefore, both with those philosophers of science who have taken it for granted that general scientific standards are immutable and reason can recognise them *a priori*,<sup>129</sup> and with those who have thought that the light of reason illuminates only particular cases. The methodology of historiographical research programmes specifies ways both for the philosopher of science to learn from the historian of science and *vice versa*.

But this two-way traffic need not always be balanced. The statute law approach should become much more important when a tradition degenerates<sup>130</sup> or a new bad tradition is founded.<sup>131</sup> In such cases statute law may thwart the authority of the corrupted case law, and slow down or even reverse the process of degeneration.<sup>132</sup> When a scientific school degenerates into pseudo-science, it may be worthwhile to force a methodological debate in the hope that working scientists will learn more from it than philosophers (just as when ordinary language degenerates into, say, journalese, it may be worthwhile to invoke the rules of grammar).<sup>133</sup>

#### D. *Conclusion*

In this paper I have proposed a ‘historical’ method for the evaluation of rival methodologies. The arguments were primarily addressed to the philosopher of science and aimed at showing how he can – and should – learn from the history of science. But the same arguments also imply that the historian of science must, in turn, pay serious attention to the philosophy of science and decide upon which methodology he will base his internal history. I hope to have offered some strong arguments for the following theses. First, each methodology of science determines a characteristic (and sharp) demarcation between (primary) internal history and (secondary) external history and, secondly, both historians and philosophers of science must make the best of the critical interplay between internal and external factors.

Let me finally remind the reader of my favourite – and by now well-worn – joke that history of science is frequently a caricature of its rational reconstructions; that rational reconstructions are frequently caricatures of actual history; and that some histories of science are caricatures both of actual history and of its rational reconstructions.<sup>134</sup> This paper, I think, enables me to add: *Quod erat demonstrandum*.

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#### NOTES

\* Earlier versions of this paper were read and criticized by Colin Howson, Alan Musgrave, John Watkins, Elie Zahar, and especially John Worrall.

The present paper further develops some of the theses proposed in my (1970). I have tried, at the cost of some repetition, to make it self-contained.

<sup>1</sup> 'Internal history' is usually defined as intellectual history; 'external history' as social history (cf. e.g. Kuhn (1968)). My unorthodox, new demarcation between 'internal' and 'external' history constitutes a considerable problemshift and may sound dogmatic. But my definitions form the hard core of a historiographical research programme; their evaluation is part and parcel of the evaluation of the fertility of the whole programme.

<sup>2</sup> This is an all-important shift in the problem of normative philosophy of science. The term 'normative' no longer means rules for arriving at solutions, but merely directions for the appraisal of solutions already there. Thus *methodology* is separated from *heuristics*, rather as value judgments are from ought statements. (I owe this analogy to John Watkins.)

<sup>3</sup> This profusion of synonyms has proved to be rather confusing.

<sup>4</sup> The epistemological significance of scientific 'acceptance' and 'rejection' is, as we shall see, far from being the same in the four methodologies to be discussed.

<sup>5</sup> 'Neo-inductivism' demands only (provably) highly probable generalisations. In what follows I shall only discuss classical inductivism; but the watered down neo-inductivist variant can be similarly dealt with.

<sup>6</sup> Cf. p. 107.

<sup>7</sup> For a detailed discussion of inductivist (and, in general, justificationist) criticism cf. my (1966).

<sup>8</sup> I am now using the term 'paradigm' in its pre-Kuhnian sense.

<sup>9</sup> This compatibility was pointed out by Agassi on pp. 23–27 of his (1963). But did he not point out the analogous compatibility within his own falsificationist historiography; cf. above, pp. 98–9.

<sup>10</sup> Cf. e.g. Bernal (1965), p. 377.

<sup>11</sup> Some logical positivists belonged to this set: one recalls Hempel's horror at Popper's casual praise of certain external metaphysical influences upon science (Hempel, 1937).

<sup>12</sup> When German obscurantists scoff at 'positivism', they frequently mean radical internalism, and in particular, radical inductivism.

<sup>13</sup> For what I here call *revolutionary conventionalism*, see my (1970), pp. 105–6 and 187–9.

<sup>14</sup> I mainly discuss here only one version of revolutionary conventionalism, the one which Agassi, in his (1966), called 'unsophisticated': the one which assumes that factual propositions – unlike pigeonhole systems – can be 'proven'. (Duhem, for instance, draws no clear distinction between facts and factual propositions.)

<sup>15</sup> It is important to note that most conventionalists are reluctant to give up inductive generalisations. They distinguish between the '*floor of facts*', the '*floor of laws*' (i.e. inductive generalisations from 'facts') and the '*floor of theories*' (or of pigeonhole systems) which classify, conveniently, both facts and inductive laws. (Whewell, the conservative conventionalist and Duhem, the revolutionary conventionalist differ less than most people imagine.)

<sup>16</sup> One may call such metaphysical principles 'inductive principles'. For an 'inductive principle' which – roughly speaking – makes Popper's 'degree of corroboration' (a conventionalist appraisal) the measure of Popper's verisimilitude (truth-content minus falsity-content) see my (1968a), pp. 390–408 and my (1971a), § 2. (Another widely spread 'inductive principle' may be formulated like this: "What the group of trained – or up-to-date, or suitably purged – scientists decide to *accept* as 'true', is true.")

<sup>17</sup> Most historical accounts of the Copernican revolution are written from the conventionalist point of view. Few claimed that Copernicus' theory was an 'inductive

generalisation' from some 'factual discovery'; or that it was proposed as a bold theory to replace the Ptolemaic theory which had been 'refuted' by some celebrated 'crucial' experiment.

For a further discussion of the historiography of the Copernican revolution, cf. my (1971b).

<sup>18</sup> For example, for non-inductivist historians Newton's '*Hypotheses non fingo*' represents a major problem. Duhem, who unlike most historians did not over-indulge in Newton-worship, dismissed Newton's inductivist methodology as logical nonsense; but Koyré, whose many strong points did not include logic, devoted long chapters to the 'hidden depths' of Newton's muddle.

<sup>19</sup> In this paper I use this term to stand exclusively for one version of falsificationism, namely for 'naive methodological falsificationism', as defined in my (1970), pp. 93–116.

<sup>20</sup> Since in his methodology the *concept* of intuitive simplicity has no place, Popper was able to use the term 'simplicity' for 'degree of falsifiability'. But there is more to simplicity than this: cf. my (1970), pp. 131ff.

<sup>21</sup> For a discussion cf. my (1970), especially pp. 99–100.

<sup>22</sup> For further discussion cf. pp. 108–09.

<sup>23</sup> Agassi (1963).

<sup>24</sup> An experimental discovery is a *chance discovery in the objective sense* if it is neither a confirming nor a refuting instance of some theory in the objective body of knowledge of the time; it is a *chance discovery in the subjective sense* if it is made (or recognised) by the discoverer neither as a confirming nor as a refuting instance of some theory he personally had entertained at the time.

<sup>25</sup> Agassi (1963), pp. 64–74.

<sup>26</sup> Within the Popperian circle, it was Agassi and Watkins who particularly emphasized the importance of unfalsifiable or barely testable '*empirical*' theories in providing *external* stimulus to later properly *scientific* developments. (Cf. Agassi, 1964 and Watkins, 1958.) This idea, of course, is already there in Popper's (1934) and (1960). Cf. my (1970), p. 184; but the new formulation of the difference between their approach and mine which I am going to give in this paper will, I hope, be much clearer.

<sup>27</sup> Popper occasionally – and Feyerabend systematically – stressed the catalytic (*external*) role of alternative theories in devising so-called 'crucial experiments'. But alternatives are not merely catalysts, which can be later removed in the rational reconstruction, they are *necessary* parts of the falsifying process. Cf. Popper (1940) and Feyerabend (1965); but cf. also Lakatos (1970), especially p. 121, footnote 4.

<sup>28</sup> Cf. Popper (1968a) and (1968b).

<sup>29</sup> The terms 'progressive' and 'degenerating problemshifts', 'research programmes', 'superseding' will be crudely defined in what follows – for more elaborate definitions see my (1968b) and especially my (1970).

<sup>30</sup> Popper does not permit this: 'There is a vast difference between my views and conventionalism. I hold that what characterises the empirical method is just this: our conventions determine the acceptance of the *singular*, not of the *universal* statements' (Popper, 1934, Section 30).

<sup>31</sup> The falsificationist hotly denies this: 'Learning from experience is learning from a refuting instance. The refuting instance then becomes a problematic instance'. (Agassi, 1964 p. 201). In his (1969) Agassi attributed to Popper the statement that 'we learn from experience by refutations' (p. 169), and adds that according to Popper one can learn *only* from refutation but not from corroboration (p. 167). Feyerabend, even in his (1969), says that '*negative instances suffice in science*'. But these remarks indicate a very

one-sided theory of learning from experience. (Cf. my (1970), p. 121, footnote 1, and p. 123.)

<sup>32</sup> Duhem, as a staunch positivist within philosophy of science, would, no doubt, exclude most 'metaphysics' as unscientific and would not allow it to have any influence on science proper.

<sup>33</sup> Cf. my (1968a), pp. 383–6, my (1968b), pp. 162–7, and my (1970), pp. 116ff. and pp. 155ff.

<sup>34</sup> Cf. Popper (1934), Section 85.

<sup>35</sup> Cf. Duhem (1906), Part II, Chapter VI, § 10.

<sup>36</sup> In fact, I define a research programme as degenerating even if it anticipates novel facts but does so in a patched-up development rather than by a coherent, pre-planned positive heuristic. I distinguish three types of *ad hoc* auxiliary hypotheses: those which have no excess empirical content over their predecessor ('*ad hoc*<sub>1</sub>'), those which do have such excess content but none of it is corroborated ('*ad hoc*<sub>2</sub>') and finally those which are not *ad hoc* in these two senses but do not form an integral part of the positive heuristic ('*ad hoc*<sub>3</sub>'). Examples for an *ad hoc*<sub>1</sub> hypothesis are provided by the linguistic prevarications of pseudosciences, or by the conventionalist stratagems discussed in my (1963–4), like 'monsterbarring', 'exceptionbarring', 'monsteradjustment', etc. A famous example of an *ad hoc*<sub>2</sub> hypothesis is provided by the Lorentz-Fitzgerald contraction hypothesis; an example of an *ad hoc*<sub>3</sub> hypothesis is Planck's first correction of the Lummer-Pringsheim formula (also cf. p. 103). Some of the cancerous growth in contemporary social 'sciences' consists of a cobweb of such *ad hoc*<sub>3</sub> hypotheses, as shown by Meehl and Lykken. (For references, cf. my (1970), p. 175, footnotes 2 and 3.)

<sup>37</sup> The rivalry of two research programmes is, of course, a protracted process during which it is rational to work in either (*or, if one can, in both*). The latter pattern becomes important, for instance, when one of the rival programmes is vague and its opponents wish to develop it in a sharper form in order to show up its weakness. Newton elaborated Cartesian vortex theory in order to show that it is inconsistent with Kepler's laws. (Simultaneous work on rival programmes, of course, undermines Kuhn's thesis of the psychological incommensurability of rival paradigms.)

The progress of one programme is a vital factor in the degeneration of its rival. If programme  $P_1$  constantly produces 'novel facts' these, by definition, will be anomalies for the rival programme  $P_2$ . If  $P_2$  accounts for these novel facts only in an *ad hoc* way, it is degenerating by definition. Thus the more  $P_1$  progresses, the more difficult it is for  $P_2$  to progress.

<sup>38</sup> Cf. especially my (1970), pp. 120–1.

<sup>39</sup> Cf. especially my (1968a), p. 385 and (1970), p. 121.

<sup>40</sup> For instance, a rival theory, which acts as an *external* catalyst for the Popperian falsification of a theory, here becomes an *internal* factor. In Popper's (and Feyerabend's) reconstruction such a theory, after the falsification of the theory under test, can be removed from the rational reconstruction; in my reconstruction it has to stay within the internal history lest the falsification be undone. (Cf. note 27.)

Another important consequence is the difference between Popper's discussion of the Duhem-Quine argument and mine; cf. on the one hand Popper (1934), last paragraph of section 18 and Section 19, footnote 1; Popper (1957b), pp. 131–3; Popper (1963a), p. 112, footnote 26, pp. 238–9 and p. 243; and on the other hand, my (1970), pp. 184–9.

<sup>41</sup> For the falsificationist this is a repulsive idea; cf. e.g. Agassi (1963), pp. 48ff.

<sup>42</sup> Feyerabend seems now to deny that even this is a possibility; cf. his (1970a) and especially (1970b) and (1971).

<sup>43</sup> I use 'verisimilitude' here in Popper's technical sense, as the difference between the truth content and falsity content of a theory. Cf. his (1963a), Chapter 10.

<sup>44</sup> For a more general discussion of this problem, cf. pp. 108–09.

<sup>45</sup> Also cf. p. 94, 96, 98, 106, 120.

<sup>46</sup> The reader should remember that in this paper I discuss only naive falsificationism; cf. note 19.

<sup>47</sup> This is Kuhn's comment on Galileo's successful *prediction* of the phases of Venus (Kuhn, 1957, p. 224). Like Mill and Keynes before him, Kuhn cannot understand why the historic order of theory and evidence should count, and he cannot see the importance of the fact that Copernicans *predicted* the phases of Venus, while Tychoonians only explained them by *post hoc* adjustments. Indeed, since he does not see the importance of the fact, he does not even care to mention it.

<sup>48</sup> Cf. note 36.

<sup>49</sup> For a statement of this position and an interesting critical discussion cf. Polanyi (1951), pp. 4ff and pp. 78ff.

<sup>50</sup> Cf. Popper (1963b) and Musgrave (1969).

<sup>51</sup> This was illustrated convincingly, by Elkana, for the case of the so-called simultaneous discovery of the conservation of energy; cf. his (1971).

<sup>52</sup> Also cf. note 47.

<sup>53</sup> For the Mertonian brand of functionalism – as Alan Musgrave pointed out to me – priority disputes constitute a *prima facie* disfunction and therefore an anomaly for which Merton has been labouring to give a general socio-psychological explanation. (Cf. e.g. Merton 1957, 1963 and 1969.) According to Merton "scientific *knowledge* is not the richer or the poorer for having credit given where credit is due: it is the social *institution* of science and individual men of science that would suffer from repeated failures to allocate credit justly" (Merton, 1957, p. 648). But Merton overdoes his point: in important cases (like in some of Galileo's priority fights) there was more at stake than institutional interests: the problem was whether the Copernican research programme was progressive or not. (Of course, not all priority disputes have scientific relevance. For instance, the priority dispute between Adams and Leverrier about who was first to discover Neptune had no such relevance: whoever discovered it, the discovery strengthened the same (Newtonian) programme. In such cases Merton's external explanation may well be true.)

<sup>54</sup> Kuhn (1970), p. 239; my italics.

<sup>55</sup> Feyerabend (1970), p. 215.

<sup>56</sup> *Ibid.*

<sup>57</sup> Cf. note 2.

<sup>58</sup> I do, of course, *not* claim that such decisions are necessarily uncontroversial. In such decisions one has to use also one's *common sense*. Common sense (that is, judgment in *particular* cases which is not made according to mechanical rules but only follows general principles which leave some *Spielraum*) plays a role in all brands of non-mechanical methodologies. The Duhemian conventionalist needs common sense to decide when a theoretical framework has become sufficiently cumbersome to be replaced by a 'simpler' one. The Popperian falsificationist needs common sense to decide when a basic statement is to be 'accepted', or to which premise the *modus tollens* is to be directed. (Cf. my (1970), pp. 106ff.) But neither Duhem, nor Popper gives a blank cheque to 'common sense'. They give very definite guidance. The Duhemian judge directs the jury of common sense to agree on comparative simplicity; the Popperian judge directs the jury to look out primarily for, and agree upon, accepted basic statements which clash with

accepted theories. My judge directs the jury to agree on appraisals of progressive and degenerating research programmes. But, for example, there may be conflicting views about whether an accepted basic statement expresses a *novel* fact or not. Cf. my (1970), p. 156.

Although it is important to reach agreement on such verdicts, there must also be the possibility of appeal. In such appeals inarticulated common sense is questioned, articulated and criticised. (The criticism may even turn from a criticism of law interpretation into a criticism of the law itself.)

<sup>59</sup> Most conventionalists have also an intermediate inductive layer of 'laws' between facts and theories; cf. note 15.

<sup>60</sup> The proposition "the Proutian programme was carried through" looks like a 'factual' proposition. But there are no 'factual' propositions: the phrase only came into ordinary language from dogmatic empiricism. *Scientific 'factual' propositions* are theory-laden: the theories involved are 'observational theories'. *Historiographical 'factual' propositions* are also theory-laden: the theories involved are methodological theories. In the decision about the truth-value of the 'factual' proposition, 'the Proutian programme was carried through,' two methodological theories are involved. First, the theory that the units of scientific appraisal are research programmes; secondly, some *specific* theory of how to judge whether a programme was 'in fact' carried through. For all these considerations a Popperian internal historian will not need to take any interest whatsoever in the *persons* involved, or in their beliefs about their own activities.

<sup>61</sup> The 'first world' is that of matter, the 'second' the world of feelings, beliefs, consciousness, the 'third' the world of objective knowledge, articulated in propositions. This is an age-old and vitally important trichotomy; its leading contemporary proponent is Popper. Cf. Popper (1968a), (1968b) and Musgrave (1969) and (1971a).

<sup>62</sup> Of course what, in this context, constitutes 'false belief' (or 'false consciousness'), depends on the rationality theory of the critic: cf. pp. 94, 96 and 98. But no rationality theory can ever succeed in leading to 'true consciousness'.

<sup>63</sup> If the publication of Bohr's programme had been delayed by a few years, further speculation might even have led to the spin problem without the previous observation of the anomalous Zeeman effect. Indeed, Compton raised the problem in the context of the Bohrian programme in his (1919).

<sup>64</sup> I first applied this expositional device in my (1963–4); I used it again in giving a detailed account of the Proutian and the Bohrian programmes; cf. my (1970), pp. 138, 140, 146. This practice was criticised at the 1969 Minneapolis conference by some historians. McMullin, for instance, claimed that this presentation may illuminate a *methodology*, but certainly not real *history*: the text tells the reader what ought to have happened and the footnotes what in fact happened (cf. McMullin, 1970). Kuhn's criticism of my exposition ran essentially on the same lines: he thought that it was a specifically *philosophical* exposition: "a *historian* would not include *in his narrative* a factual report which he knows to be false. If he had done so, he would be so sensitive to the offence that he could not conceivably compose a footnote calling attention to it." (Cf. Kuhn, 1970, p. 256.)

<sup>65</sup> Cf. L. P. Williams (1970).

<sup>66</sup> Perhaps I should emphasize the difference between on the one hand, *inductivist historiography of science*, according to which *science* proceeds through discovery of hard facts (in nature) and (possibly) inductive generalisations, and, on the other hand, the *inductivist theory of historiography of science* according to which *historiography of science* proceeds through discovery of hard facts (in history of science) and (possibly)

inductive generalisations. 'Bold conjectures', 'crucial negative experiments', and even 'progressive and degenerating research programmes' may be regarded as 'hard historical facts' by some inductivist historiographers. One of the weaknesses of Agassi's (1963) is that he omitted to emphasize this distinction between scientific and historiographical inductivism.

<sup>67</sup> Cf. Popper (1957b), Section 31.

<sup>68</sup> This thesis implies that the work of those 'externalists' (mostly trendy 'sociologists of science') who claim to do social history of some scientific discipline without having mastered the discipline itself, and its internal history, is worthless. Also cf. Musgrave (1971a).

<sup>69</sup> Unfortunately there is only one single word in most languages to denote history<sub>1</sub> (the set of historical events) and history<sub>2</sub> (a set of historical propositions). Any history<sub>2</sub> is a theory and value-laden reconstruction of history<sub>1</sub>.

<sup>70</sup> That is, a hypothesis  $h$  is scientific only if there is a number  $q$  such that  $p(h, e) = q$  where  $e$  is the available evidence and  $p(h, e) = q$  can be proved. It is irrelevant whether  $p$  is a Carnapian confirmation function or a Popperian corroboration function as long as  $p(h, e) = q$  is allegedly proved. (Popper's third note on corroboration, of course, is only a curious slip which is out of tune with his philosophy: cf. my (1968a), pp. 411–7.)

Probabilism has never generated a programme of historiographical reconstruction; it has never emerged from grappling – unsuccessfully – with the very problems it created. As an epistemological programme it has been degenerating for a long time; as a historiographical programme it never even started.

<sup>71</sup> Popper (1934), Sections 11 and 85. Also cf. the comment in my (1971a), footnote 13.

The methodology of research programmes too is, in the first instance, defined as a game; cf. especially pp. 99–100.

<sup>72</sup> This whole problem area is the subject of my (1968a), pp. 390ff, but especially of my (1971a).

<sup>73</sup> Cf. Popper (1934), Sections 4 and 11. Popper's definition of science is, of course, his celebrated 'demarcation criterion'.

<sup>74</sup> For an excellent discussion of the distinction between nominalism and realism (or, as Popper prefers to call it, 'essentialism') in the theory of definitions, cf. Popper (1945), vol. II, chapter 11, and (1963a), p. 20.

<sup>75</sup> Popper (1934), Section 11.

<sup>76</sup> *Ibid.*

<sup>77</sup> Popper (1934), Section 4. But Popper, in his *Logik der Forschung* never specifies a purpose of the game of science that would go beyond what is contained in its rules. The thesis that the aim of science is truth, occurs only in his writings since 1957. All that he says in his *Logik der Forschung* is that the quest for truth may be a psychological motive of scientists. For a detailed discussion cf. my (1971a).

<sup>78</sup> This flaw is the more serious since Popper himself has expressed qualifications about his criterion. For instance in his [1963a] he describes 'dogmatism', that is, treating anomalies as a kind of 'background noise', as something that is 'to some extent necessary' (p. 49). But on the next page he identifies this 'dogmatism' with 'pseudoscience'. Is then pseudoscience 'to some extent necessary'? Also, cf. my (1970), p. 177, footnote 3.

<sup>78a</sup> Cf. Popper (1963), pp. 33–7.

<sup>79</sup> Popper (1934), Section 29.

<sup>80</sup> This approach, of course, does not imply that we believe that the scientists' basic judgments' are unfailingly rational; it only means that we accept them in order to criticise universal definitions of science. (If we were to add that no such universal

definition has been found and no such *universal* definition will ever be found, the stage would be set for Polanyi's conception of the lawless closed autocracy of science.)

My meta-criterion may be seen as a 'quasi-empirical' self-application of Popperian falsificationism. I introduced this 'quasi-empiricalness' earlier in the context of mathematical philosophy. We may abstract from *what* flows in the logical channels of a deductive system, whether it is something certain or something fallible, whether it is truth and falsehood or probability and improbability, or even moral or scientific desirability and undesirability: it is the *how* of the flow which decides whether the system is negativist, 'quasi-empirical', dominated by *modus tollens* or whether it is justificationist, 'quasi-Euclidean', dominated by *modus ponens*. (Cf. my (1967).) This 'quasi-empirical' approach may be applied to *any* kind of normative knowledge: Watkins has already applied it to ethics in his (1963) and (1967). But now I prefer another approach: cf. note 122.

<sup>81</sup> It may be noted that this metacriterion does not have to be construed as psychological, or 'naturalistic' in Popper's sense. (Cf. his (1934), Section 10.) The definition of the 'scientific *elite*' is not simply an empirical matter.

<sup>82</sup> Popper (1963a), p. 38, footnote 3; my italics. This, of course, is equivalent to his celebrated 'demarcation criterion' between [internal, rationally reconstructed] science and non-science (or 'metaphysics'). The latter may be [externally] 'influential' and has to be branded as pseudoscience only if it declares itself to be science.

<sup>83</sup> Cf. my (1970), pp. 100–1.

<sup>84</sup> Cf. e.g. his (1934), Section 18.

<sup>85</sup> Cf. my (1970), especially pp. 135ff.

<sup>86</sup> *Ibid.*, pp. 138ff.

<sup>87</sup> Cf. Popper (1934), Section 24.

<sup>88</sup> Cf. my (1970), especially pp. 140ff.

<sup>89</sup> In general Popper stubbornly overestimates the immediate striking force of purely verbal criticism. "Once a mistake, or a contradiction, is pinpointed, there can be no verbal evasion: it can be proved, and that is that" (Popper, 1959, p. 394). He adds: "Frege did not try evasive manoeuvres when he received Russell's criticism." But of course he did. (Cf. Frege's *Postscript* to the second edition of his *Grundgesetze*.)

<sup>90</sup> Interestingly, as Kuhn points out, "a consistent interest in historical problems and a willingness to engage in original historical research distinguishes the men [Popper] has trained from the members of any other current school in the philosophy of science" (Kuhn 1970, p. 236). For a hint at a possible explanation of the apparent discrepancy cf. note 129.

<sup>91</sup> For instance, he claims that a perpetual motion machine would 'refute' (on his terms) the first law of thermodynamics (1934, Section 15). But how can one interpret, on Popper's own terms, the statement that '*K* is a perpetual motion machine' as a 'basic', that is, as a spatio-temporally singular statement?

<sup>92</sup> I am referring to Feyerabend's (1970) and (1971).

<sup>93</sup> Cf. Popper (1934), Section 30 and Popper (1945), Vol. II, pp. 220–1. He stressed that Einstein's problem was how to account for experiments 'refuting' classical physics and he "did not... set out to criticise our conceptions of space and time." But Einstein certainly did. His Machian criticism of our concepts of space and time, and, in particular his operationalist criticism of the concept of simultaneity played an important role in his thinking.

I discussed the role of the Michelson-Morley experiments at some length in my (1970).

Popper's competence in physics would never, of course, have allowed him to distort the history of relativity theory as much as Beveridge, who wanted to persuade economists to an empirical approach by setting them Einstein as an example. According to Beveridge's falsificationist reconstruction, Einstein 'started [in his work on gravitation] from facts [which refuted Newton's theory, that is,] from the movements of the planet Mercury, the unexplained aberrancies of the moon' (Beveridge, 1937). Of course, Einstein's work on gravitation grew out from a 'creative shift' in the positive heuristic of his special relativity programme, and certainly not from pondering over Mercury's anomalous perihelion or the moon's devious, unexplained aberrancies.

<sup>94</sup> Popper (1963a), pp. 220, 239, 242–3 and (1963b), p. 965. Popper, of course, is left with the problem why 'counterexamples' (that is, anomalies) are not recognised immediately as causes for rejection. For instance, he points out that in the case of the breakdown of parity "there had been many observations – that is, photographs of particle tracks – from which we might have read off the result, but the observations had been either ignored or misinterpreted" (1963b, p. 965). Popper's – external – explanation seems to be that scientists have not yet learned to be sufficiently critical and revolutionary. But is not it a better – and internal – explanation that the anomalies *had* to be ignored until some progressive alternative theory was offered which turned the counterexamples into examples?

<sup>95</sup> *Op. cit.*, p. 246.

<sup>96</sup> As I mentioned, one Popperian, Agassi, did write a book on the historiography of science (Agassi, 1963). The book has some incisive critical sections flogging inductivist historiography, but he ends up by replacing inductivist mythology by falsificationist mythology. For Agassi *only* those facts have scientific (internal) significance which can be expressed in propositions which conflict with some extant theory: only their discovery deserves the honorific title 'factual discovery'; factual propositions which *follow from* rather than *conflict with* known theories are irrelevant; so are factual propositions which are *independent of* them. If some valued factual discovery in the history of science is known as a confirming instance or chance discovery, Agassi boldly predicts that on *close* investigation they will turn out to be refuting instances, and he offers five case-studies to support his claim (pp. 60–74). Alas, on *closer* investigation it turns out that Agassi got wrong all the five examples which he adduced as confirming instances of his historiographical theory. In fact all the five examples (in our normative meta-falsificationist sense) 'falsify' his historiography.

<sup>97</sup> Cf. Duhem (1906), Popper (1948) and (1957), Agassi (1963).

<sup>98</sup> Of course, an inductivist may have the temerity to claim that genuine science has not yet started and may write a history of extant science as a history of bias, superstition and false belief.

<sup>99</sup> Cf. Popper (1934), Section 19.

<sup>100</sup> Cf. Polanyi (1951), p. 70.

<sup>101</sup> Kuhn (1957). Also cf. Price (1959).

<sup>102</sup> Cohen (1960), p. 61. Bernal, in his (1954), says that "[Copernicus's] reasons for [his] revolutionary change were essentially philosophic and aesthetic [that is, in the light of conventionalism, scientific];" but in later editions he changes his mind: "[Copernicus's] reasons were mystical rather than scientific."

<sup>103</sup> For a more detailed sketch cf. my (1971b).

<sup>104</sup> Other types of criticism of methodologies may, of course, be easily devised. We may, for instance, apply the standards of each methodology (not only falsificationism) to itself. The result, for most methodologies, will be equally destructive: inductivism

cannot be proved inductively, simplicity will be seen as hopelessly complex. (For the latter cf. end of note 106.)

<sup>105</sup> Cf. Polanyi (1958), Kuhn (1962), Holton (1969), Feyerabend (1970) and (1971). I should also add Lakatos (1963–4), (1968b), and (1970).

<sup>106</sup> Kuhn (1957). Such historiographical criticism can easily drive some rationalists into an irrational defence of their favourite falsified rationality theory. Kuhn's historiographical criticism of the simplicity theory of the Copernican revolution shocked the conventionalist historian Richard Hall so much that he published a polemic article in which he singled out and re-asserted those aspects of Copernican theory which Kuhn himself had mentioned as possibly having a claim to higher simplicity, and ignored the rest of Kuhn's – valid – argument (Hall, 1970). No doubt, simplicity can always be defined for *any* pair of theories  $T_1$  and  $T_2$  in such a way that the simplicity of  $T_1$  is greater than that of  $T_2$ .

For further discussion of conventionalist historiography cf. my (1971b).

<sup>107</sup> Thus Polanyi is a conservative rationalist concerning science, and an 'irrationalist' concerning the philosophy of science. But, of course, this meta-'irrationalism' is a perfectly respectable brand of rationalism: to claim that the concept of 'scientifically acceptable' cannot be further defined, but only transmitted by the channels of 'personal knowledge', does not make one an outright irrationalist, only an outright conservative. Polanyi's position in the philosophy of natural science corresponds closely to Oakeshott's ultra-conservative philosophy of political science. (For references and an excellent criticism of the latter cf. Watkins (1952)). Also cf. pp. 120–122.

<sup>108</sup> Of course, none of the critics were aware of the exact logical character of meta-methodological falsificationism as explained in this section and none of them applied it completely consistently. One of them writes: 'At this stage we have not yet developed a general theory of criticism even for scientific theories, let alone for theories of rationality: therefore if we want to falsify methodological falsificationism, we have to do it before having a theory of how to do it' (Lakatos, 1970, p. 114).

<sup>109</sup> I used the critical machinery developed in this paper against Feyerabend's epistemological anarchism in my (1971b).

<sup>110</sup> Kuhn's vision was criticised from many quarters; cf. Shapere (1964 and 1967) Scheffler (1967) and especially the critical comments by Popper, Watkins, Toulmin, Feyerabend and Lakatos – and Kuhn's reply – in Lakatos and Musgrave (1970). But none of these critics applied a systematic *historiographical* criticism to his work. One should also consult Kuhn's 1970 *Postscript* to the second edition of his (1962) and its review by Musgrave (Musgrave, 1971b).

<sup>111</sup> Cf. Feyerabend (1970a, 1970b and 1971); and Kuhn (1970).

<sup>112</sup> For instance, one may refer to the actual immediate impact of at least *some* 'great' negative crucial experiments, like that of the falsification of the parity principle. Or one may quote the high respect for at least *some* long, pedestrian, trial-and-error procedures which occasionally precede the announcement of a major research programme, which in the light of my methodology is, at best, 'immature science'. (Cf. my (1970), p. 175; also cf. L. P. Williams's reference to the history of spectroscopy between 1870 and 1900 in his (1970)). Thus the judgment of the scientific élite, on occasions, goes also against *my* universal rules too.

<sup>113</sup> There is a certain analogy between this pattern and the occasional appeal procedure of the theoretical scientist against the verdict of the experimental jury; cf. my (1970), pp. 127–31.

<sup>114</sup> This latter criterion is analogous to the exceptional 'depth' of a theory which clashes

with some basic statements available at the time and, at the end, emerges from the clash victoriously. (Cf. Popper's, 1957a) Popper's example was the inconsistency between Kepler's laws and the Newtonian theory which set out to explain them.

<sup>115</sup> Conventionalism, of course, had performed this historic role to a great extent before Popper's version of falsificationism.

<sup>116</sup> Van der Waerden had thought that the Bohr-Kramers-Slater theory was bad: Popper's theory showed it to be good. Cf. Van der Waerden (1967), p. 13 and Popper (1963a), pp. 242ff; for a critical discussion cf. my (1970), p. 168, footnote 4 and p. 169, footnote 1.

<sup>117</sup> The attitude of some modern logicians to the history of mathematics is a typical example; cf. my (1963-4), p. 3.

<sup>118</sup> This formulation was suggested to me by my friend Michael Sukale.

<sup>119</sup> Cf. my (1970), Section 3(c).

<sup>120</sup> Cf. my (1970), pp. 138-73.

<sup>121</sup> Duhem himself gives only one explicit example: the victory of wave optics over Newtonian optics (1906), Chapter VI, § 10 (also see Chapter IV, § 4). But where Duhem relies on intuitive 'common sense', I rely on an analysis of rival problemshifts (cf. my (1972)).

<sup>122</sup> One may introduce the notion of '*degree of correctness*' into the meta-theory of methodologies, which would be analogous to Popper's empirical content. Popper's empirical 'basic statements' would have to be replaced by quasi-empirical 'normative basic statements' (like the statement that 'Planck's radiation formula is arbitrary').

Let me point out here that the methodology of research programmes may be applied not only to norm-impregnated historical knowledge but to any normative knowledge, including even ethics and aesthetics. This would then supersede the naive falsificationist 'quasi-empirical' approach as outlined on Note 80.

<sup>123</sup> Cf. text to note 9. (The term 'wild speculation' is, of course, a term inherited from inductivist methodology. It should now be reinterpreted as 'degenerating programme'.)

<sup>124</sup> The fact that even degenerating externalist theories have been able to achieve some respectability was to a considerable extent due to the weakness of their previous internalist rivals. Utopian Victorian morality either creates false, hypocritical accounts of bourgeois decency, or adds fuel to the view that mankind is totally depraved; utopian scientific standards either create false, hypocritical accounts of scientific perfection, or add fuel to the view that scientific theories are no more than mere beliefs bolstered by some vested interests. This explains the 'revolutionary' aura which surrounds some of the absurd ideas of contemporary sociology of knowledge: some of its practitioners claim to have unmasked the bogus rationality of science, while, at best, they exploit the weakness of outdated theories of scientific rationality.

<sup>125</sup> For examples cf. Cantor (1971) and the Forman-Ewald debate (Forman, 1969 and Ewald, 1969).

<sup>126</sup> I call '*historiographical positivism*' the position that history can be written as a completely *external* history. For historiographical positivists history is a purely empirical discipline. They deny the existence of objective standards as opposed to mere beliefs about standards. (Of course, they too hold beliefs about standards which determine the choice and formulation of their historical problems.) This position is typically Hegelian. It is a special case of *normative positivism*, of the theory that sets up might as the criterion of right. (For a criticism of Hegel's ethical positivism cf. Popper (1945), Vol. I, pp. 71-2, Vol. II, pp. 305-6 and Popper (1961).) Reactionary Hegelian obscurantism

pushed values back completely into the world of facts; thus reversing their separation by Kantian philosophical enlightenment.

<sup>127</sup> Kuhn seems to be in two minds about objective scientific progress. I have no doubt that, being a devoted scholar and scientist, he *personally* detests relativism. But his *theory* can either be interpreted as denying scientific progress and recognising only scientific change; or, as recognising scientific progress but as 'progress' marked solely by the march of actual history. Indeed, on his criterion, he would have to describe the catastrophe mentioned in the text as a proper 'revolution'. I am afraid this might be one clue to the unintended popularity of his theory among the New Left busily preparing the 1984 'revolution'.

<sup>128</sup> The technical term 'Euclidean' (or rather 'quasi-Euclidean') means that one starts with universal, high level propositions ('axioms') rather than singular ones. I suggested in my (1967) and (1962) that the 'quasi-Euclidean' versus 'quasi-empirical' distinction is more useful than the '*a priori*' versus '*a posteriori*' distinction.

Some of the 'apriorists' are, of course, empiricists. But empiricists may well be apriorists (or, rather, 'Euclidean') on the meta-level here discussed.

<sup>129</sup> Some might claim that Popper does *not* fall into this category. After all, Popper defined 'science' in such a way that it should include the refuted Newtonian theory and exclude unrefuted astrology, Marxism and Freudianism.

<sup>130</sup> This seems to be the case in modern particle physics; or according to some philosophers and physicists even in the Copenhagen school of quantum physics.

<sup>131</sup> This is the case with some of the main schools of modern sociology, psychology and social psychology.

<sup>132</sup> This, of course, explains why a good methodology – 'distilled' from the mature sciences – may play an important role for immature and, indeed, dubious disciplines. While Polanyiite academic autonomy should be defended for departments of theoretical physics, it must not be tolerated, say, in institutes for computerised social astrology, science planning or social imagistics. (For an authoritative study of the latter, cf. Priestley (1968).)

<sup>133</sup> Of course, a critical discussion of scientific standards, possibly leading even to their improvement, is impossible without articulating them in general terms; just as if one wants to challenge a language, one has to articulate its grammar. Neither the conservative Polanyi nor the conservative Oakeshott seem to have grasped (or to have been inclined to grasp) the *critical* function of language – Popper has. (Cf. especially Popper (1963a), p. 135).

<sup>134</sup> Cf. e.g. my (1962), p. 157 or my (1968a), p. 387, footnote 1.

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