to the belief that quantum mechanics, the second great revolutionary theory of the century, must outdo the Einsteinian revolution, especially with respect to its epistemological depth. It seems to me that this belief affected some of the great founders of quantum mechanics,[56] and also some of the great founders of molecular biology.[57] It led to the dominance of a subjectivist interpretation of quantum mechanics; an interpretation which I have been combating for almost forty years. I cannot here describe the situation; but while I am aware of the dazzling achievement of quantum mechanics (which must not blind us to the fact that it is seriously incomplete[58]) I suggest that the orthodox interpretation of quantum mechanics is not part of physics, but an ideology. In fact, it is part of a modernistic ideology; and it has become a scientific fashion which is a serious obstacle to the progress of science.

XIV

I hope that I have made clear the distinction between a scientific revolution and the ideological revolution which may sometimes be linked with it. The ideological revolution may serve rationality or it may undermine it. But it is often nothing but an intellectual fashion. Even if it is linked to a scientific revolution it may be of a highly irrational character; and it may consciously break with tradition.

But a scientific revolution, however radical, cannot really break with tradition, since it must preserve the success of its predecessors. This is why scientific revolutions are rational. By this I do not mean, of course, that the great scientists who make the revolution are, or ought to be, wholly rational beings. On the contrary: although I have been arguing here for the rationality of scientific revolutions, my guess is that should individual scientists ever become 'objective and rational' in the sense of 'impartial and detached', then we should indeed find the revolutionary progress of science barred by an impenetrable obstacle.

above) uses most questionable epistemological arguments *against* Newton's absolute space and *for* a very important theory.

[56] Especially Heisenberg and Bohr.

[57] Apparently it affected Max Delbrück; see *Perspectives in American history*, Vol. 2, Harvard University Press (1968). Émigré physicists and the biological revolution, by Donald Fleming, pp. 152–189, especially sections iv and v. (I owe this reference to Professor Mogens Blegvad.)

[58] It is clear that a physical theory which does not explain such constants as the electric elementary quantum (or the fine structure constant) is incomplete; to say nothing of the mass spectra of the elementary particles. See my paper, Quantum mechanics without 'the observer', referred to in note 45 above.

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V

HISTORY OF SCIENCE AND ITS RATIONAL RECONSTRUCTIONS

IMRE LAKATOS

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 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.[1]

From *PSA 1970, Boston Studies in the Philosophy of Science VIII*, edited by R. C. Buck and R. S. Cohen, pp. 91–108. Reprinted by permission of D. Reidel Publ. Co., Dordrecht, Holland. Part 2 of this paper is pp. 109–37, in that volume followed by comments by H. Feigl, R. Hall, N. Koertge, and T. S. Kuhn, and a reply by Lakatos. The complete paper is also printed in [51], vol. i, pp. 109–38.

[1] 'Internal history' is usually defined as intellectual history; 'external history' as social history (cf. e.g. Kuhn [8, pp. 105–26]). 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.

1 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.[2] Often these rules, or systems of appraisal, also serve as 'theories of scientific rationality', 'demarcation criteria' of 'definitions of science'. 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 characterized by rules governing the (scientific) *acceptance* and *rejection* of theories or research programmes.[3] These rules have a double function. Firstly, 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 generalizations from them.[4] 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.

[2] 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.)

[3] 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.

[4] '*Neo*-inductivism' demands only (provably) highly probable generalizations. In what follows I shall only discuss classical inductivism; but the watered down neo-inductivist variant can be similarly dealt with.

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 proccupied 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.[5]

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.[6] 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 *generalizations*. 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.[7] The main paradigms of inductivist historiography were Kepler's generalizations from Tycho Brahe's careful observations; Newton's discovery of his law of gravitation by, in turn, inductively generalizing Kepler's 'phenomena' of planetary motion; and Ampère's discovery of his law of electrodynamics by inductively generalizing 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

[5] Cf. *below*, pp. 114–115.

[6] For a detailed discussion of inductivist (and, in general, justificationist) criticism cf. my [50].

[7] I am now using the term 'paradigm' in its pre-Kuhnian sense.

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,[8] indeed, some vulgar-Marxists identify major phases in the history of science with the major phases of economic development.[9] 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 'metaphisical') 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;[10] but since external influences always exist, radical internalism is utopian, and, as a theory of rationality, self-destructive.[11]

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 organizes 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

[8] This compatibility was pointed out by Agassi [52, pp. 23–7]. But did he not point out the analogous compatibility within his own falsificationist historiography; cf. *below*, pp. 114–15.

[9] Cf. e.g. Bernal, J. D., *Science in History* (3rd edn. London: Watts, 1965). p. 377.

[10] Some logical positivists belonged to this set: one recalls Hempel's horror at Popper's casual praise of certain external metaphysical influences upon science. Cf. C. G. Hempel's review of [40], *Deutsche Literaturzeitung* 1937, pp. 309–14.

[11] When German obscurantists scoff at 'positivism', they frequently mean radical internalism, and in particular, radical inductivism.

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.[12] 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;[13] the *changes* on the theoretical level are merely instrumental. Theoretical 'progress' is only in convenience ('simplicity'), and not in truth-content.[14] 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.[15] 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

[12] For what I here call *revolutionary conventionalism*, see [50, pp. 104 and 187–9].

[13] I mainly discuss here only one version of revolutionary conventionalism, the one which Agassi called 'unsophisticated': the one which assumes that factual propositions—unlike pigeonhole systems—can be 'proven'. Cf. Agassi, J., 'Sensationalism', *Mind*, 75 (1966) pp. 1–24. (Duhem, for instance, draws no clear distinction between facts and factual propositions.)

[14] It is important to note that most conventionalists are reluctant to give up inductive generalizations. They distinguish between the '*floor of facts*', the '*floor of laws*' (i.e. inductive generalizations from 'facts') and the '*floor of theories*' (or of pigeonhole systems) which classify, conveniently, both facts and inductive laws. (Whewell [46], the conservative conventionalist, and Duhem [47], the revolutionary conventionalist, differ less than most people imagine.)

[15] 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 [51, i, ch. 3, sec. 2, and ii, pp. 181–93]. (Another widely held '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.')

version of conventionalism. On the other hand some conventionalists did not have sufficient logical education to realize 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 Bergsonians' 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.[16] 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 derive 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.[17]

[16] Most historical accounts of the Copernican revolution are written from the conventionalist point of view. Few claimed that Copernicus' theory was an 'inductive generalization' 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. [51, I, ch. 4].

(c) Methodological falsificationism

Contemporary falsificationism arose as a logico-epistemological criticism of inductivism and of Duhemian conventionalism. Inductivism was criticized 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 criticized 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 [40] proposed a new 'falsificationist' methodology.[18] 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 not intuitive simplicity complicate the picture.[19]

[17] 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 (e.g. [14]), whose many strong points did not include logic, devoted long chapters to the 'hidden depths' of Newton's muddle.

[18] *In this paper I use this term to stand exclusively for one version of falsificationism, namely for 'naive methodological falsificationism', as defined in* [50, pp. 93–114].

[19] 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. [50, pp. 132 ff.].

(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 assumptions. [Cf. 50, p. 98] 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. [Cf. 51, i, 121-2].)

The Popperian historian looks for great, 'bold', falsifiable theories and for great negative curicial experiments. These form the skeleton of his 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 [52, pp. 64–74]. 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).[20] 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.

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 these are cornerstones of Popper's *psychology*

[20] 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 recognized) by the discoverer neither as a confirming nor as a refuting instance of some theory he personally had entertained at the time.

of discovery.[21] Feyerabend developed another interesting *psychological* thesis of Popper's, namely, that proliferation of rival theories may–*externally*–speed up *internal* Popperian falsification.[22]

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 [45, chs. 3, 4]. 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 great 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.[23] 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.

[21] Within the Popperian circle, it was Agassi and Watkins who particularly emphasized the importance of unfalsifiable or barely testable *'metaphysical'* theories in providing an *external* stimulus to later properly *scientific* developments. (Cf. [55]) and Watkins, J. W. M., 'Influential and Confirmable Metaphysics', *Mind*, n.s. 67 (1958), pp. 344–65. The idea, of course, is already there in Popper's [40]. Cf. [50, p. 183]; 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.

[22] 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 [44] and Feyerabend [63]; but cf. also [50, p. 121, n. 4].

[23] The terms 'progressive' and 'degenerating problemshifts', 'research programmes' 'superseding' will be crudely defined in what follows–for more elaborate definitions see [50].

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.[24] 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 primarly the positive heuristic of his programme, not the anomalies, which dictate the choice of his problems.*[25] 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.[26]

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. (Cf. [50, pp. 116 ff. and 154 ff.] and [51, ii, pp. 175–8].) 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—

[24] 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 [40], sec. 30).

[25] The falsificationist hotly denies this: learning from experience is learning from a refuting instance. The refuting instance then becomes a problematic instance' (Agassi [55], p. 201). In [56] 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 says that *'negative instances suffice in science'*. But these remarks indicate a very one-sided theory of learning from experience. (Cf. [50, pp. 121–3]).

[26] Duhem [47], 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.

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 [40, sec. 85]—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 to judge when a 'framework' is to be abandoned [cf. 47, II. vi. 10], 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'*).[27] If a research programme progressively explains more than a rival, it 'supersedes' it, and the rival can be eliminated (or, if you wish, 'shelved').[28]

[27] 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$_1$'*), those which do have such excess content but none of it is corroborated (*'ad hoc$_2$'*) 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$_3$'*). Examples of *ad hoc$_1$* hypotheses are provided by the linguistic prevarications of pseudosciences, or by the conventionalist stratagems discussed in my [48] like 'monsterbarring', 'exceptionbarring', 'monsteradjustment', etc. A famous example of an *ad hoc$_2$* hypothesis is provided by the Lorentz–Fitzgerald contraction hypothesis; an example of an *ad hoc$_3$* hypothesis is Planck's first correction of the Lummer–Pringsheim formula (also cf. 51, i, p. 79 ff). Some of the cancerous growth in contemporary social 'sciences' consists of a cobweb of such *ad hoc$_3$* hypotheses, as shown by Meehl and Lykken. (For references, cf. [50, p. 176, n. 1]).

[28] 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

(*Within* a research programme a theory can only be eleminated 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 [50, pp. 122-3]; empirical 'falsification' and actual 'rejection' become independent. 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.[29])

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.[30]

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

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.

[29] 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. *above*, n. 22).

Another important consequence is the difference between Popper's discussion of the Duhem-Quine argument and mine; cf. on the one hand Popper's [40, last para. sec. 18, and sec. 19, n. 1], [43, pp. 131-3], and [44, p. 112, n. 26, pp. 238-9 and 243]; and on the other hand [50, pp. 184-9].

[30] For the falsificationist this is a repulsive idea; cf. e.g. Agassi [54], pp. 48 ff.

about its defeat. Thus pigheadedness, like modesty, has more 'rational' scope. *The scores of the rival sides, however, must be recorded*[31] *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.[32] 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 [cf. 51, II, pp. 121-2].)

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 problemshifts. 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. When 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 1950s, 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 1960s. 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.

But the methodology of research programmes draws a demarcation

[31] Feyerabend seems now to deny that even this is a possibility; cf. [49 and 67].

[32] I use 'versimilitude' here in Popper's technical sense, as the difference between the truth content and falsity content of a theory. Cf. his [44, ch. 10].

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 *pre*dictions 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.[33] 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'.[34] 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 '*ad hoc*$_3$' theory (cf. *above*, n. 27). 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 its arises.[35] 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

[33] The reader should remember that in this paper I discuss only naive falsificationism; cf. *above*, n. 18.

[34] This is Kuhn's comment on Galileo's successful *pre*diction of the phases of Venus [2, 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 *pre*dicted the phases of Venus, while Tychonians 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.

[35] For a statement of this position and an interesting critical discussion cf. Polanyi M., *The Logic of Liberty* (London: Routledge and Kegan Paul, 1951), pp. 4 ff. and pp. 78 ff.

(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. The discovery of a new problem, however, may not be so readily explicable. If one thinks of the history of science as composed 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.[36]

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, the 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 (cf. *above*, n. 34) or that Cartesians managed to explain everything that the Newtonians *pre*dicted—but only *post hoc*. Newtonian optical theory explained *post hoc* many phenomena which were anticipated and first observed by Huyghensians.[37]

[36] This was illustrated convincingly, by Elkana, for the case of the so-called simultaneous discovery of the conservation of energy (Y. Elkana, 'The conservation of energy, a case of simultaneous discovery?', *Archives internationales d'Histoire des Sciences*, 24 (1971), pp. 31-60.)

[37] 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. [84], [85], [86]). 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

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 criticized 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*'. 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)' [49, p. 215]. Since I specify no such time limit, Feyerabend concludes that my standards are no more than *'verbal ornaments'*. A related points 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. (Cf. *above*, n.2.) 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.[38]

failures to allocate credit justly' [84, 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.)

[38] I do, of course, *not* claim that such decisions are necessarily uncontroversial. In such decisions one has also to use 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. [50, p. 106 ff.]). 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. [50, p.70].

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 criticized. (The criticism may even turn from a criticism of law interpretation into a criticism of the law itself.)

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 generalizations. The internal history of *conventionalists* consists of factual discoveries and of the erection of pigeonhole systems and their replacement by allegedly simpler ones.[39] The internal history of *falsificationists* dramatizes 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*

[39] Most conventionalists have also an intermediate inductive layer of 'laws' between facts and theories; cf. *above*, no. 14.

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, whther 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[40] 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.[41] 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

[40] (Lakatos used Prout as an example in [50, pp. 138–40]. The 'Proutian programme' starting from 1815 tried to show that all atomic weights are integral multiples of the weight of hydrogen. See article VI *below*, p. 140–41.–*Editor*).

[41] 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.

will regard this historical fact as a fact in the second world which is only a caricature of its counterpart in the third world.[42] *Why* such caricatures come about is none of his business; he might–in a footnote –pass on to the externalist the problem of why certain scientists had 'false beliefs' about what they were doing.[43]

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 be judged only 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.[44] (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.[45]

[42] 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 [45, ch. 3, 4].

[43] Of course what, in this context, constitutes 'false belief' (or 'false consciousness'), depends on the rationality theory of the critic. But no rationality theory can ever succeed in leading to 'true consciousness'.

[44] 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 1919.

[45] I first applied this expositional device in [48]; I used it again in giving a detailed account of the Proutian and the Bohrian programmes, cf. *above*, no.40. This practice was criticized 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 [29]. Kuhn's

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 philosophizing 'we need a lot more examples'.[46] But such an inductivist theory of historiography is utopian.[47] *History without some theoretical 'bias' is impossible.*[48] Some historians look for the discovery of hard facts, inductive generalizations, 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 wider popular acclaim over an incredibly long period or why a 'progressive problemshift' was left 'unreasonably' unacknowledged.[49] 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.[50] This being so, the hitherto neglected problem of appraising rival logics of scientific discovery and, hence, rival reconstructions of history, acquires paramount importance.

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. [49, p. 256].)

[46] Cf. L. Pearce Williams in [49].

[47] 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 generalizations, 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 generalizations. '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 [54] is that he omitted to emphasize this distinction between scientific and historiographical inductivism.

[48] Cf. Popper [43, sec. 31].

[49] 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.

[50] Unfortunately there is only one single word in most languages to denote $history_1$ (the set of historical events) and $history_2$ (a set of historical propositions). Any $history_2$ is a theory- and value-laden reconstruction of $history_1$.

(*The paper continues with part 2*, 'Critical comparison of *methodologies*: history as a test of its rational reconstruction' [51, i, pp. 121–38].)

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