The Philosophy of Progress...

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1. Introduction

Philosophical dialogue is a curious activity. Arguments are expected to be rigorous, but no demand is made that there must be evidence for the premisses. Terminology is expected to be precise, but its appropriateness to the subject matter under discussion can be left unexplored. Officially, nothing is conceded; but, in fact, a great deal is taken for granted. *Ad argumentum* mingles indiscriminately with *ad hominem*; and, above all, the evidential warrant for one's philosophical claims is, like the topics of sex and religion to the less enlightened, one of those delicate issues never to be discussed in mixed company.

Such conventions as these that are associated with contemporary philosophical exchange make it difficult to have a balanced discussion about *Progress and Its Problems*. That work is, in the first instance, a descriptive model of theory change in science. It purports to establish what sorts of factors have *in fact* influenced scientific decision-making. It seeks to fit scientific activity into an aim-theoretic structure which relates the presumed goals of science to the actual methods which scientists utilize. First and foremost, then, the book offers a descriptive theory of how science develops. As such, and here arises the first difficulty philosophers have with it, the model ought to be assessed by asking whether it fits the facts it purports to describe. Because epistemologists and philosophers of science are notoriously short on facts, they feel uncomfortable about taking the book on its own terms.

Instead, they want to read it chiefly as a work in abstract epistemology and normative methodology of science. By casting it in this more familiar guise, they can proceed to ascertain whether it is epistemically sound, a process that ultimately reduces to asking whether my largely implicit epistemological prejudices coincide with their explicit ones.

PSA 1978, Volume 2, pp. 530-547

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192488 Published online by Cambridge University Press

Where my central concern is with the question, "Is science what I claim it to be?", their major preoccupation is with the very different question, "Ought science to be what Laudan claims it is?" For the purposes of this volume, I am prepared to play the game by the philosophical rules and to pretend that the normative question is the central one. Indeed, I will even grant that Progress and Its Problems makes philosophical claims and that it is entirely appropriate to explore its philosophical consequences.

But I must confess that I am taken aback from time to time by the frequency with which philosophical commentators dismiss my work because it is not congenial to their favorite theory of knowledge. Uncongenial it may be; but if it is what it purports to be (i.e., a reasonably faithful account of the factors that go into scientific decision-making), then its ill-fit with an epistemology raises far more doubts about the soundness of the relevant epistemology than about the credentials of the model itself.

2. Some Dubious Dependencies

It is a characteristic feature of human thought, and a symptom of the narrowness of the human imagination, that we chronically take what are at best sufficient conditions to be necessary and sufficient ones. Intellectual progress, more often than not, consists in showing that there is more than one way to skin a cat, i.e., in showing that what were once regarded as necessary and sufficient conditions are merely sufficient and not necessary. Such victories are never won quickly, however; acknowledging that we can sometimes get what we want without having to affect what we always believed was its sine qua non is usually accompanied by an awkward process of re-evaluation. It is precisely that process which Progress and Its Problems sought to provoke.

When I wrote the book, I was persuaded that there were several goals to which most philosophers of science assumed science subscribed, and for each of these goals there was a widely acknowledged and purportedly unique means of achieving it. Unfortunately, the means in question are not available to us. Accordingly, I wanted to explore whether some of these traditional goals could be achieved in other ways, ways which represented options genuinely available to us.

Let me be specific. The following theses seem to be widely believed:

(a) scientific realism presupposes epistemic optimism;
(b) scientific progress presupposes the cumulative retention of all the explanatory successes of earlier theories within later ones;
(c) scientific rationality presupposes a successive convergence on the truth;
(d) scientific objectivity presupposes the permanent applicability of a fixed set of scientific methods and criteria of theory

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192488 Published online by Cambridge University Press

evaluation;

(e) comparative theory assessment presupposes full translational commensurability between rival theories;

(f) warranted acceptability presupposes legitimate grounds for believing that which is acceptable.

Because my book severed these time-honored linkages, many readers have been appalled at the abandon with which I have torn asunder what God himself hath joined together. The trouble with these familiar presuppositional patterns is that, in literally every case, a historical study of science reveals that the second element is not generally achieved in real cases of theory change. Thus, we do not have any reason for believing that scientific theories are true; we know that successor theories do not always--or even usually--retain all the explanatory successes of their predecessors; there is no account of convergence on the truth which leads one to believe that science has that asymptotic character; the methods of science, both implicit and explicit ones, do change through time and are not permanent; we know that many rival theories are not fully translatable into one another's or a common language; and we know that scientists frequently accept theories long before (if such a time ever comes) the evidence for those theories warrants believing them to be true.

These results are not mere tentative hunches; they are conclusions that follow inescapably from much of the empirical study of scientific history during the last four decades. If there is anything that is reliably established by historical scholarship, it is claims of the kind I have just enumerated.

One possible moral to draw from this depressing picture is that actual science is neither progressive nor rational nor objective nor acceptable nor realistic. But this move--which one might call the Feyerabend gambit--is neither the only nor the most appropriate response to this state of affairs. What I tried to show in Progress and Its Problems (and elsewhere[2]) is that all these alleged 'dependencies' involve mistakenly regarding admittedly sufficient conditions as necessary ones. I wanted to show, for instance, that one could talk about the progress of science even in the absence of a cumulativity condition (specifically, by requiring later theories to explain a larger number of important problems than their predecessors, even if they did not solve all the problems of their predecessor). I showed that we could assess the relative empirical support for rival theories even if we had no grounds for believing they approached ever closer to the truth.

I do not have space here to review all those interconnections. I shall focus instead on the two which have given my commentators the most grief, specifically (a) and (d) above.

2.1 Sceptical Realism. To suggest, as I do, that the primary aim of science might be other than acquiring presumptively true beliefs is to run squarely against the grain of most traditions which have been

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192488 Published online by Cambridge University Press

dominant in epistemology since antiquity. But the universally acknowledged fact of the matter is that there are no compelling reasons whatever to believe that scientific theories, past or present, are true (or likely or verisimilar, or any of the other ersatz surrogates for truth). To retain the view that science aims at presumptively true theories, in the face of the admission that we would not know how to recognize a true theory if we had it, is to render science an irrational enterprise; for, on any coherent account of what rational behavior is, it is irrational to adopt a goal which (a) we do not know how to achieve, (b) we could not recognize if we had achieved, and (c) was such that we could not even tell whether we were gradually moving closer to achieving it. "True scientific theories" seems to be precisely such a goal.[3]

Confronted by this dilemma, many philosophers have argued that science aims, not at the truth itself, but at ever-closer approximations to it. (Peirce, Reichenbach and Popper are obvious instances.) What vitiates this approach is the absence of any plausible criterion for ascertaining that one theory is more truthlike or more verisimilar than another. Hence attributing this slightly weaker aim to science--every bit as much as the earlier, stronger ones of truth and certainty--leads to the view that science is a utopian, and therefore irrational, activity whose telos is, to the best of our knowledge, forever beyond our grasp.[4]

Unwilling to face up squarely to the charge that their approach makes all scientific theorizing irrational, proponents of truth and truth-likeness as the aim of science engage chronically in the diversionary tactic of accusing anyone who would deny that truth is the aim of science of being an 'instrumentalist', as if scientific realism and the pursuit of true knowledge were inseparably intertwined.[5] Even if they were so intertwined, this move would not get the "truth school" off the hook, since, at least judging by the current state of the art, that school has no coherent reply to the charge that it renders all scientific theorizing irrational.

In what follows, however, I want to show that some of us who deny that presumptively true theories should be the goal of science are in fact scientific realists rather than instrumentalists. But it is crucial to stress that, regardless of the outcome of that exploration, it is to no avail to rant and rail against instrumentalism when what is at issue is whether a truth-oriented approach is even marginally applicable to scientific theorizing.

The question before us now, therefore, is this: is it correct that a realistic (as opposed to an instrumentalistic) construal of scientific theories requires us to believe that theories, so construed, are true, probable, verisimilar, etc.? I take it that--at a minimum--a scientific realist is committed to the following:

(i) scientific theories have truth values;
(ii) scientific theories must be logically consistent;

(iii) the (non-logical) terms in a scientific theory (including 'non-observational' terms) are intended to refer to, and to make claims about, states in the world (i.e., instrumentalist 'cleansings' of theories à la Craig or Ramsey do not capture the full range of existential claims of a theory);

(iv) 'deep structure' theories are methodologically preferable to phenomenological ones.

Taken together, these four claims--at the semantic, logical, referential and methodological level--constitute a recognizable (and probably the only viable) form of scientific realism. As I showed in Progress and Its Problems, such a realism is entirely compatible with the epistemically sceptical claim that we may never have legitimate grounds for saying that a theory is true, probable or verisimilar. In short, epistemic scepticism and scientific realism are fully compatible; those who--and this includes many readers of my book[6]--have asserted that epistemic scepticism is inevitably instrumentalist in character have ignored the fact that realism is chiefly a semantic thesis about what theories are intended to assert, rather than an epistemic one about our warranted confidence in theories. Realism is thus fully compatible with a variety of epistemological stances, including my own.

The position I have outlined above is the view of theories contained in Progress and Its Problems. My view of theories is not full-blown 'scientific realism' (à la Sellars or Grover Maxwell), for I do not share the epistemological optimism of bloated realism, believing the theory of knowledge of contemporary realism to be ill-suited to an understanding of science. But it simply will not do to suggest, as several critics have, that my work is committed to an instrumentalist view of theories. In every respect except the narrowly epistemological one, my construal of theories is precisely that of realism.

Indeed, it is incomprehensible to me that a reader of my book could imagine that I was anything but a realist. The book stresses repeatedly the importance of deep-structure theories, the crucial role of theoretical entities, and the positive role of metaphysical explorations in science. I produce inductive evidence that deep-structure theories have been more effective problem-solvers than phenomenological ones and that, accordingly, such theories conduce to achieve (what I view as) the aims of science; something which none of the truth-oriented realists has ever managed to show with respect to what they take as the aims of science.

Other confusions on this issue abound. For instance, Mellor asserts that unless our goal is true theories, we have no grounds for taking the entailments of a theory seriously since "all entailment does is pass on a theory's truth". Mellor's argument here simply confuses a theory's possessing a truth value with our knowing what that truth value is. On the prevailing philosophy of logic, entailment relations can obtain between sentences so long as they have truth values. Hence to invoke the machinery of entailment (as I do in my definition of when a theory solves a problem), all I need require is that a theory is true or false;

I do not need the additional and gratuitous requirement that we must know which truth value it has.[7]

Needless to say, I cannot and do not dismiss the possibility that someone may someday offer an account of truth or truthlikeness that makes it possible to say warrantedly of certain theories that they are true or nearly so. And I for one would be delighted. But that (remote) possibility, even if actualized, does not militate against the following claims:

(i) on existing accounts, there is no reason to believe that scientific theories are true, or nearly so;
(ii) a realistic construal of theories is not necessarily wedded to the view that science aspires to certifiably true or demonstrably verisimilar theories;
(iii) no proponent of presumptively true or truthlike theories as the aim of science has yet been able to show how science, or anything like it, could be a rational activity.

2.2 Objectivity and the Evolution of Rational Methodology. For a very long time, philosophers and logicians of science believed that there was a set of rules and principles which constituted sound ampliative or scientific reasoning. They believed that these rules and principles were everywhere implicit in scientific practice, even if the explicit preaching of scientists about methodological matters had shifted through time. The adherence to these procedural rules was thought to be essential to the objectivity of science, since every scientist would have to submit his favorite theories to the systematic and searching critique required by the scientific method.

The scholarship of the last three generations (and much of this research was initiated by Pierre Duhem's pioneering studies) has knocked into a cocked hat the view that scientific method and rationality have always been what we now take them to be. On almost every conceivable front, the principles constitutive of scientific rationality have undergone significant changes. Views about the aims of science have changed; methods for handling experimental error have shifted drastically; the probative significance attached to various types of confirming and disconfirming instances has varied enormously (for instance, during many periods in the history of science, the capacity of a theory to make successful, surprising predictions was not emphasized); the perceived role of various 'non-evidential' factors in theory appraisal (e.g., simplicity, economy, exhibition of analogies, use of quantitative concepts, etc.) has vacillated perceptibly through time.[8]

But many modern philosophers who know these facts are still loathe to accept them. (Witness Lakatos' efforts to attribute these shifts to 'false consciousness' on the part of scientists. Witness, too, the fact that Koertge--prior to her recent 'conversion' after PSA, 1978--clung desperately to the hope that the principles of rationality did not change.) What explains the reluctance of most philosophers to admit that rational methodology does change is a subscription by them to the belief that scientific objectivity presupposes the permanence of scientific methods and criteria for the evaluation of theories. Only so long as we have a common yardstick for evaluating theories, they say, can we guarantee that theory choice will be objective. If every scientist can pick his own methodology and principles of evidence, what is to protect us from that hopeless relativism in which each scientist simply chooses a methodology which would make his theory look good?

The thrust of Koertge's madhouse example is to bring home precisely this point. But the example is singularly ill-chosen, for it completely misconstrues my account of rationality. It is one thing to insist, as I do, that many methodological principles and rules gradually change with time. It is quite another matter to say, and I do not, that one can rationally adopt whatever methodology one likes. The fact that not all methods are permanent in science, the fact that new methodologies occasionally come to the fore, does not warrant the claim that all methodologies are equally good. Within a given context, it will be readily ascertainable that some methodological principles are sounder, better argued for, more persuasively established than others.

Just as scientists must decide what is the best available explanation for certain facts they want to explain, so too must they ascertain what the best available methodology is. It may not always remain in that privileged position any more than 'best explanations' do--for one of the things we learn with time is how to test our theories more efficaciously--but so long as a methodology does enjoy such a status, it must be decisive in adjudicating scientific disagreements. Objectivity, as between rival theories, is insured by insisting that they be judged according to the best available methodology (even though one acknowledges that it may not permanently remain so).

If this approach of mine is to work, we obviously must be able to formulate criteria for ascertaining what the best available methodology or philosophy of science is. Koertge is right in that, but I do not understand how she can possibly charge me with having ignored this problem. A major part of chapter five of Progress and Its Problems is devoted to a discussion of how to choose the best available methodology. If she had read that far, she would have seen that I make very detailed proposals on this question. (See especially [4], pp. 158-163.) One may or may not find my specific suggestions viable (and I have not the space to detail them here). But for her to say that I "do not explain how one methodology can be better than another" is to deny that I have addressed what was one of the central foci of the book.

3. Problem Solving

Whether a problem-solving approach to science will ultimately prove to be sound depends on our collective capacity to make good on several promissory notes. We need a coherent account of how problems are generated, and for that we may need to utilize and further develop the resources of erotetic logic. We need unambiguous criteria for individuating problems and for weighting their relative importance;

Bayesian approaches may help here, although they generally seem to beg more questions than they clarify. It would be nice, as well, to have a heuristics for searching for problem solutions; work in artificial intelligence and cognitive psychology may be instructive here. The model at present deals only with deductivistic problem-solving; no statistical version of it has yet been developed. In these and many other respects, what we have therefore is a sketch of a model of scientific growth rather than a fully-developed model.

But for all its sketchiness, it is not so ambiguous as some of my fellow symposiasts suspect. Some of the apparent ambiguities are due to my infelicities of style and presentation, but several others simply reflect a refusal by my critics to attend to the structure of my arguments. In this section of the paper, I want to straighten out some of the latter confusions.

3.1 When Does a Theory Solve a Problem? Within most accounts of the philosophy of science, a very sharp distinction is drawn between the instances which a theory explains and the instances which confirm it. In many of the cruder versions (e.g., Hempel's account of 'qualitative confirmation'), a theory is confirmed by anything it entails but (on the prevailing view) most theories never explain what they entail. This is because, on the prevailing account, a theory can be said to be explanatory only if it satisfies some very demanding epistemic and pragmatic strictures (e.g., the theory must be true, highly probable or--more recently--the 'best available' theory). Since very few theories satisfy those demands, we are forced to say that most theories--although widely confirmed--do not explain anything. This is certainly an allowable characterisation of the concept of explanation (even if it is radically at odds with how scientists use the term); but it is not the only characterisation. I find it more illuminating to say that a theory explains (or, as I prefer, 'solves') all those instances (problems) which are positive evidence for it. Since these will ordinarily (except in the case of statistical problem-solving) be precisely those instances whose description it entails, I claim that a theory solves a problem just in case it entails, in conjunction with initial conditions, a description of the putative state of affairs which poses the problem. Thus, in ascertaining whether a theory provides a solution to a problem, we need not decide whether the theory is true or probable, or even the best available solution to it.

Mellor and Koertge are unhappy with this analysis because, as Koertge puts it, it does not tell us when we have "a good solution... or the solution" to a problem. They think that, because I put all (deductivistic) solutions on a par, I have to countenance all solutions as equally plausible. This is simply false. My primary concern is with appraising theories. To appraise a theory, I need to know which problems it solves. If I adopted the Koertge-Mellor suggestions, I would never be able to tell which theories to accept because my acceptance measure is a function of problem-solving success. If we could not ascertain whether a theory solved a problem until we knew that it gave 'a good' or 'the best' solution to it, we would obviously be involved

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192488 Published online by Cambridge University Press

in a vicious circle.

Prioritizing choices must be made. Where the classical tradition (from Aristotle to Hempel) made the decision about whether a theory, T, explains an event, E, parasitic upon a prior determination of the well-foundedness of T, my approach involves a reversal of those priorities, making a decision about the well-foundedness of T parasitic upon the problems which T solves and/or explains. Apart from its other advantages, my approach comes a great deal closer to scientific practice than the traditional analysis does. Thus, modern-day scientists are quite prepared to grant that, for instance, Ptolemy's theory explained retrograde motion, that Newton's theory solved the problem of the oblate sphericity of the earth, and that Lyell had a mechanism for explaining climate change. All these theories are now regarded as false, yet no working scientist would challenge the claim that they each explained/solved a wide range of problems. Equally, my characterisation preserves the universal intuition that (as yet unrefuted) rival theories can be said to offer solutions to certain problems; prevailing models of explanation do not; for if they are genuine rivals, they cannot all satisfy the epistemic requirements imposed by such models. Moreover, I can say, for instance, that Einstein's theory offers a <u>better</u> solution to the problem of free fall than does Newton's theory; but all the 'better' here conveys is that Einstein's theory is more strongly supported than Newton's, by virtue of its comparative over-all problem-solving effectiveness.

My concern here is not to establish--with respect to this particular issue--the superiority of my approach over more familiar ones. All I need show is that I have consistent and unambiguous machinery for ascertaining when a deductively-formulated theory solves a problem. I do have that in the concept of entailment. Such a view of problem solving has none of the catastrophic, 'anything goes' features which are conjured up by Koertge's madhouse example and by her suggestion that, on my account, "every crank is successful."

3.2 <u>Conceptual Problems Revisited</u>. One of the central respects in which my account of science departs from most prevailing ones is in its insistence that scientific theories are frequently judged in terms of their compatibility with existing philosophical systems. In particular, I stressed the regulative and substantive roles which metaphysical and epistemological analyses play in the development and evaluation of scientific theories. Koertge claims that my analysis here is both unoriginal and erroneous. That others before me have acknowledged <u>some</u> role for metaphysics in science, I readily admit (indeed, I refer to all the 'precursors' she cites). But Koertge's glib treatment of this issue--as of some others--glosses over important differences. With Popper and Agassi, for instance, the role of metaphysics is exclusively the <u>heuristic</u> one of providing a kind of inspiration to scientific theorizing. Once we have particularized the metaphysics into a testable theory, the former is allowed to play no evaluational or adjudicatory role in appraising the latter. In Lakatos, the role of metaphysics is even more attenuated. He calls the hard cores of his research programmes 'metaphysical', meaning to convey by that term nothing more than the arbitrarily-imposed irrefutability of certain sentences comprising our theories (as Koertge, who has written at length on Lakatos's work, must know perfectly well). This Pickwickean sense of the place of metaphysics in science has nothing whatever to do with the claims I make about the role of metaphysics, classically construed, in science.

In brief, I argued that scientific theories are frequently assessed in terms of their compatibility with theories about the nature of substances and their properties, and in terms of what they presuppose about causation and activity. The role of metaphysics in science, on my account, is not primarily heuristic and inspirational; it is critical and evaluative. In contrast to Popper, who argues that science progresses only when the empirical content of a theory is increased, I have claimed that the clarification or elimination of conceptual, metaphysical problems plays a central role in scientific progress. Where Popper, Agassi and Koertge see the transition from metaphysical foundations to 'positive' science as a mark of scientific maturity, I show that metaphysical concerns have a permanent and enduring role in the on-going process of theory evaluation.

Koertge thinks the place of metaphysics in science has been "overstressed". Such impressions are difficult to grapple with, short of rehearsing the volumes of evidence which historians of science have produced in the last three decades to show the influence of metaphysics on science. It is perhaps sufficient here to summarize that literature by remarking that most scientific thinkers (including Galileo, Descartes, Newton, Harvey, Boyle, Leibniz, Euler, Lyell, Darwin, Einstein and Planck--to name only a few) engaged in extensive investigations of the metaphysical foundations of their science; and by observing that there is no major scientific revolution in which metaphysical concerns were less prominent than straightforwardly evidential considerations. So far as I am aware, my model is the only one which assigns an unambiguous and positive role to the critical functioning of metaphysics of precisely the kind which historians have been painstakingly documenting since the 1930's.

How, as good empiricists, can we countenance such a role for metaphysics? The answer is straightforward. Like science, good metaphysics is designed to make sense of, and is ultimately tested in terms of, our experience. Metaphysical theories gain rational currency precisely to the extent they are sustained by, and capable of giving intelligibility to, a wide range of experience. The fact that metaphysics is grounded in experience makes it no less metaphysical; but that grounding does explain why scientists do, and should, attend very carefully to conflicts between their scientific theories and the best available doctrines of metaphysics. Similar remarks apply to Koertge's hand-waving dismissal of the role of theological, social and political philosophies in the appraisal of science. In so far (and only in so far) as the latter are empirically well-founded disciplines--and at some points in history each of them have been--it is entirely appropriate to bring them to bear on the appraisal of scientific theories.

3.3 Demarcation as a Pseudo-Problem. Koertge and Mellor--like many other readers of my book--voice dismay that I do not treat science as *sui generis*. My claim that rationality in science is not different in kind from rationality in certain other forms of intellectual activity usually not regarded as scientific (e.g., philosophy, history, theology, social theory) disappoints those who, with Popper, believe that the central task of the philosophy of science is to explain what is unique about *scientific* knowledge.

It is true enough that I find nothing in the aims and methods of what we commonly call the sciences which are not utilized in a wide range of apparently 'non-scientific' disciplines. For instance, many disciplines--within as well as outside the sciences--utilize hypothetico-deductive inference; many insist that theories must be checked against the data; many require that assertions about the world must be falsifiable. More generally, my claim is that the cognitive aims and the evaluative methods exhibited in common by such sciences as physics, chemistry and physiology show up across a very broad spectrum of intellectual disciplines; I am aware of no characterisation of 'scientific' knowledge which applies to all and only the disciplines we ordinarily call the 'sciences'. To those who believe otherwise, my challenge is a simple one: show me some goals which are unique to science; show me methods of investigation utilized by all the sciences and none of the non-sciences; show me rules of theory evaluation invoked only by scientists; identify some patterns of explanation, inference or testing which characterize just those things we regard as science. (I fully grant that there are some 'methods' utilized in certain sciences which are not utilized outside 'the sciences'. But there are *no* such methods common to all the major sciences, so they could not possibly be constitutive of scientific rationality.) Until and unless such a challenge is met, I shall remain unmoved by pious exhortations to the effect that demarcating science from non-science is a fundamental desideratum for epistemology.

The important distinction, so far as I am concerned, is not between scientific and non-scientific knowledge, but rather that between well-founded claims and ill-founded ones. There are many theories or bodies of doctrine that lie outside the sciences--and that includes much of 'common sense', branches of philosophy and history, certain forms of literary and aesthetic theory--which utilize perfectly respectable, empirical and conceptual criteria for evaluating rival doctrines. Attempting to argue that such forms of inquiry use methods or have cognitive aims different from those of the sciences is just silly.

That science is not different in kind from many other forms of cognitive activity need not blind us to the important differences of degree. By and large, the sciences have made more rapid progress than non-scientific disciplines; concensus is generally reached more quickly within science than outside it. The problem-solving model offers machinery for precisely characterizing the rate of progress in various

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192488 Published online by Cambridge University Press

disciplines. The 'sciences' have doubtless been the most progressive extant versions of knowledge (provided one counts mathematics and logic as sciences), but--in the absence of any decisive distinction between the 'scientific' and the 'non-scientific'--our central concern should be with characterizing knowledge in all its forms and with finding ways of advancing it. Making out an (artificial) difference in principle between scientific and other forms of knowledge only obscures rather than clarifies the central problems of the theory of knowledge.

3.4 Noretta in Wonderland. I have dealt *en passant* above with what strike me as the *prima facie* substantive points which Koertge raises against my approach. Additionally, however, her essay is replete with mistaken characterizations both of my position and of the problems confronting us. Although I do not have time to deal with all of them in detail, I want to note for the record some important confusions which her essay exhibits:

1) Although her essay is entitled "In Praise of Truth...", (which makes it sound, of course, as if she is on the side of the angels), nowhere in it does she articulate--let alone cogently defend--the view that science aims at realizably true theories. Here, as elsewhere, she offers pious handwaving and sloganeering where arguments and evidence are called for.
2) Koertge imagines that any technical difficulties accompanying measures of content will inevitably confront any measure of solved problems. This is simply false. To see why, let me first review the most notorious difficulty confronting content measures. Popper and Lakatos both insisted that we should judge the merits of rival theories by examining the size of their respective content classes, denoting this process by the rule "prefer theories with greater content". Unfortunately, the content classes of all theories are of transfinite cardinality. Hence under most circumstances *we cannot tell* whether one theory has greater content than another. Indeed, such comparisons can be made just in case one theory entails *all* the consequences of the other, a situation which rarely if ever obtains between actual scientific theories. Clearly, it is the infinite size of the classes being compared that produces these acute difficulties (and also generates the unrealistic Popper-Lakatos demand for cumulative content retention).[9]

   By contrast, the theory appraisal measure I have proposed involves the comparison of *finite* sets of sentences whose members can be enumerated and whose respective sizes can be judged utilizing the rule "prefer theories which solve the larger number of problems." Why is the number of *solved* problems finite? To speak of those problems whose solution can be credited to a theory is to refer to those *already observed* states of affairs which the theory entails. The membership of this set will always be finite, even though theories entail an infinite number of consequences, since only a finite number of the consequences can be examined. Thus, when we compare the number of problems actually (as opposed to potentially) solved by two rival theories, we are comparing sets of finite cardinality and we can meaningfully compare the respective sizes of the sets. Koertge is simply mis-using my terminology when she says that, on my view, "each theory solve[s] an infinite number of problems."[10]

3) Koertge claims it to be "unreasonable to suggest, as Laudan does, that the only way to judge a philosophical theory is by its 'empirical success' in accounting for historical facts." Nowhere in the book do I make such a claim. I do argue at length that such empirical authentication is a necessary condition for a sound philosophy of science: I do not make it a sufficient condition. There is all the difference in the world between the two.

4. Drawing the Line Between 'Richness' and Rationality

Robert Westman's essay raises a number of interesting and important challenges for my analysis. Unlike Mellor and Koertge, who believe I have adopted too tolerant a view of what rationality is, Westman chides me for having an account of rationality which is not tolerant enough.

Before I deal with the substantive issues that separate the two of us, I want to clarify my intentions. Contrary to Westman's suggestion, I am not committed to the view that man is always or even usually a rational agent; indeed, what is remarkable, when one considers all the forces conducing him to act irrationally, is that he manages to make rational choices at all. Equally, my concern has not been to develop a theory of man, but rather to explore what sorts of things count as good reasons. Impoverished accounts of scientific rationality, such as Popper's or Carnap's, make a great many things--like the whole of the history of science--seem to be irrational when they often are not. The model of scientific progress I sketched in the first half of Progress and Its Problems was designed to show that a much larger class of decisions, beliefs and actions are rational than we have imagined.

But it remains the case--and this reverts to my earlier claim, contra-Koertge--that the model does not countenance every theoretical manoeuvre as rational. Where attempts to explain behavior rationally break down, then we must utilize other explanatory strategies--often of a psychological or socio-economic kind--to make sense of what is going on. Westman is right that when confronted with competing, rival rational and non-rational accounts of the same data, my preference--for which good reasons can be given--is for the explanation in terms of good reasons. But he is simply wrong when he says that I would limit the explanatory resources of the historian entirely to modes of rational explanation. Quite the opposite. Where these fail to work, which is often, one must utilize all the explanatory resources at our disposal.

Westman's treatment of the Copernicus case betrays an important equivocation about what is involved in the rational explanation of a belief or any other cognitive action. To identify it, I want to begin by drawing a distinction between cognitive and non-cognitive goals.

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Typical instances of the former would include understanding, true belief, conceptual simplicity, reliable knowledge, and the like.[11] Non-cognitive goals could include everything from a large salary to a vigorous sex life to the respect of one's peers. Now, to be rational is, minimally, to perform those actions which one believes will conduce to achieving one's goals. To be cognitively rational is to perform those actions which one believes will conduce to one's cognitive goals.

With these distinctions in hand, we can turn to the case of the early Copernicans. I have claimed that it was rational for them to pursue and work on the Copernican theory, but that it was then unreasonable--in the light of the available evidence and arguments--to accept heliocentrism as the best available account of the cosmos. Westman agrees, I believe, that the evidence and arguments of the period did not warrant acceptance of that theory. He then proceeds to suggest that non-cognitive motivations explain the early reception of the Copernican theory (e.g., Rheticus wanted a father figure and the opponents of Copernicus did not want to offend the natural philosophers). Both these historical conjectures may be correct, but I am not persuaded that Westman has shown us that the agents behaved in a cognitively rational way. If someone engages in a cognitive act (e.g., accepting or rejecting a theory, pursuing a new line of research, believing the results of an experiment to be sound) in order to promote non-cognitive goals or desires, then his behavior is both cognitively inappropriate and cognitively irrational. Accepting Copernicus' theory may well have promoted Rheticus' goal of acquiring a father figure, but his action is cognitively unreasonable since what one believes or accepts ought not be decided with respect to non-cognitive goals. More generally, it is irrational to accept or reject a belief on the grounds that doing so promotes one's non-cognitive interests. (The fact that accepting Lysenko's theories in Stalinist Russia might enhance one's career prospects is not a cognitively sound reason for being a Lysenkoist!)

I do not deny that people sometimes behave in this way. But if someone engages in cognitive acts exclusively to serve non-cognitive ends, I am not prepared to regard such an end/means relationship as either appropriate or rational. If the early Copernicans accepted the Copernican theory as true when all that was cognitively warranted was the (weaker) pursuit of that theory, then they were behaving inappropriately and irrationally.

As a form of *reductio* of my position, Westman alleges that my approach would lead us to give higher marks to the anti-Copernican Jesuits and to the Tychonists than to the early Copernicans. He is probably right, for I can see no cognitively cogent reason for accepting the Copernican rather than the Tychonic system in the 16th century. But why should this result be regarded as an argument against my model? The fact that the Copernican theory eventually proved itself to be better than the Tychonic one offers no reason to believe that it was rational to accept it from the beginning. Westman and Feyerabend are

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both wrong in thinking that a theory of scientific rationality is flawed if it cannot show that theories which we judge with hindsight to be good must have been warrantable as worthy of acceptance from their inception. What it is rational to accept is obviously a function of the kind of evidence and arguments that are available at a given time. As the latter change--which they did dramatically in the Copernican case--so does one's appraisal of what it is cognitively rational to accept.

Westman is right: on my reckoning, it was more rational, in the 16th century, to accept the Aristotelian, Ptolemaic and Tychonic theories than the Copernican one. Just as, to take a case from our own time, it was irrational to accept Wegener's theory of continental drift in the 1930's, even though we now find it rational to accept a rather similar theory.[12] But these judgments about acceptance do not foreclose the possibility of exploring or pursuing theories that are not yet worthy of acceptance. The weaker rationality of theory pursuit, which I have described at length, leaves scope for the development of embryonic theories without our weakening the strong demands that are appropriate when it comes to what we ought to accept. If this distinction between contexts of acceptance and pursuit seems to involve having my cake and eating it, too, that is precisely what it was designed to do. What it was not designed to do was to render cognitively rational the premature acceptance of a theory. Since Westman faults my analysis for failing to do that, he should explain why he thinks it should.

What I am generally groping for is a mean between two extremes. Unlike Mellor and Koertge, I am not willing to accept a philosophy which entails that science is entirely irrational; but unlike Westman, I want a theory of rationality with enough teeth in it to avoid the assumption that science is always rational.

5. Conclusion

I said at the beginning that, for the purposes of this symposium, I would suspend disbelief about the priority of epistemic analysis. That suspension can no longer be sustained. It is a commonplace among philosophers of physics or biology that one must first understand what physics or biology is before one engages in an epistemic and logical critique of its foundations. By contrast, many epistemologists and methodologists of science apparently believe that, prior to an empirical investigation of what science really is and how it actually works, they can settle all sorts of fundamental issues, ranging from the aims and methods of science to detailed matters of scientific inference and theory succession. The presumption seems to be that we can decide a priori what our epistemological first principles should be and can then utilize those to legislate what good science must be like. Leaving aside the unbecoming arrogance which such an approach exhibits, it rests on a very shaky view of the nature of philosophy and of the relation between philosophy and science.

My own belief is that, until and unless we have learned how science actually works, epistemological posturing will necessarily remain both ill-informed and inconclusive. Prolegomenal to any coherent theory of knowledge must be an empirical investigation of how we actually learn about the world. Those epistemologists and philosophers of science (particularly the avowed empiricists among them) who think otherwise are obliged to tell us what warrant they have for subscribing to the theories of knowledge which they espouse. In the meantime, I will continue to maintain that the first task before us is that of detailing what science is and how it functions.

Notes

[1]I am grateful to a variety of colleagues, including R. Burian and L. Krüger, for their helpful comments on earlier versions of this essay. I also want to acknowledge my gratitude to A. Grünbaum whose discussions on the issues treated here have been invaluable.

[2]See especially Laudan [6].

[3]In criticizing my account of the goals of science, Ernest Nagel notes that nothing in my analysis provides "any ground for rejecting the hypothesis that on some matters and in some circumstances science does in fact arrive at the truth." ([8], p. 317). He is right, but his argument is to no avail. Unless we can ascertain <u>which</u> of our various theories are true (and Nagel concedes we cannot) then the quest for true theories is quixotic. The quest might unknowingly be successful (e.g., some of our theories might be true), but unless we can tell whether we have succeeded, the quest remains an irrational one.

[4]It is more than a little curious that Mellor, who chides me for giving up on truth, concludes his own essay by saying that truth is redundant and that all we need be concerned with is 'belief'. If discarding truth really amounts (as Mellor says) to "depriving" science of any "philosophical interest", then replacing the older requirement of 'justified true belief' by that of 'shared belief' must have similar consequences. If, as the first part of his paper urges, Mellor wants true theories as the aim of science, then he must tell us what reliable means we have for achieving that goal; but if shared beliefs rather than truth is the aim--as he suggests towards the end--then he should make it clear how he gets truth any more centrally into the picture than I do.

[5]For a remarkably vivid example of the lack of clarity which some philosophers bring to a discussion of instrumentalism and realism, see the review of [4] by Jardine in [2].

[6]For instance, Gutting [1], Jardine [2], Koertge and Mellor.

[7]In a similar vein, Gary Gutting [1] has suggested that only if truth is our aim can we have any objection to logically inconsistent

theories. But I have a straightforward explanation of the demand for consistency: because inconsistent theories entail every statement, they will always be confronted by as many anomalies as solved problems. Replacement of one inconsistent theory by another could thus never count as progress with respect to problem-solving effectiveness.

[8]For a discussion of some of the more important of these changes, see my [3] and [5].

[9]I have not discussed here the equally devastating, recent demonstration that all false theories have the same empirical content. Accordingly, the replacement of one false theory by another cannot possibly lead to increasing verisimilitude.

[10]Gutting makes a similar mistake in his [1].

[11]What I am calling cognitive goals are not unlike what I. Levi has called 'epistemic utilities'. C.G. Hempel was, I believe, the first to introduce the idea of assessing epistemic utilities.

[12]For a documentation of this case, see R. Laudan [7].

https://doi.org/10.1086/psaprocbienmeetp.1978.2.192488 Published online by Cambridge University Press

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