# PROGRESS IN SCIENCE

(Abstract)

While Popper believes that only in science can we say that we have made genuine progress, Kuhn suggests a significant parallelism between normal science and theology. Since different paradigms solve in fact different problems, Kuhn cannot give any sense to the notion that scientific development is a growth process. But although theories with different cores cannot be compared at the object level, they can be compared at the metalevel of theoretical discourse. The idea that there is a ground plan for scientific progress goes the way of the idea that a creator is guiding the direction of organic evolution. The fact that scientists who subscribe to radically different theories about the world may not understand each other does not preclude the possibility — if they share certain metalevel goals — that they may nevertheless be able to agree on the relative cognitive success and failure of their respective theories. The criteria of progress, regardless of how we formulate them, are normative; they are not something discovered about science but something postulated in advance. The asymmetry between the past and the future of scientific research has much greater methodological significance than a merely logical asymmetry between verification and falsification.

# PROGRESS IN SCIENCE

Joseph GRUNFELD

## I

In his *Structure of Scientific Revolutions* (1) Kuhn remarks that surprisingly little is known about why science — our surest example of sound knowledge — progresses as it does. Important changes have occurred in Western man's conception of what is real and those changes are manifest in the contemporary thinking of scientists. Empiricist epistemology since Hume has been guided by the idea that observation provides a maximally certain knowledge, a foundation that provides the basic premisses for all our reasoning and without which there could not even be any probable knowledge. But if one looks at the actual scientific inquiry one sees that sense experience does not function as the foundation of knowledge at all. Instead it serves as a stimulus, provoking us to make revisions in our system of beliefs — revisions from which our observation claims are not themselves immune. Observation claims and the question what the facts are derive their credibility from background assumptions. Thus there is no sharp distinction between observation claims and theoretical claims, every claim in one way or another transcends what is presently and directly experienced (2). The principles of empiricism are presented against the background assumption that a person's perceptions are unaffected by the beliefs he has, by the assumptions he makes about the objects he is observing. Kuhn argues that because there is no such thing as a theoretically neutral description of anything, the choice of a new theory is a decision to adopt a different native language and to deploy it in a correspondingly different world.

(1) University of Chicago Press 1962; 2nd ed. 1970.

(2) MORICK, Harold (ed.) *Challenges to Empiricsm*, Wadsworth Publishing Co. Belmont, California, 1972, p. 16.

He points out that both «falsification» and «refutation» are antonyms of «proof.» They are drawn principally from logic and from formal mathematics. Invoking these terms implies the ability to compel assent from any member of the relevant professional community. Any explanation of progress in science must thus be in psychological or sociological terms in the opinion of Kuhn. *The Structure of Scientific Revolutions* contains many suggestions, some explicit, others implicit in the choice of language, of a significant parallelism between normal science and theology. He believes that the rationality of science presupposes the acceptance of a common framework. For Popper, on the other hand, a critical discussion and a comparison of various frameworks is always possible. Only in science can we say that we have made genuine progress, that we know more than we did before ([3]). In Popper's view scientific change is rational or at least rationally reconstructible. For Kuhn, scientific change — from one «paradigm» to another — is a gestalt switch. The whole idea that the rationality of science has somehow to be saved is based on a misconception. When theoretical constructions in science contradict rational common sense (as in the case of bended space or quanta jumps), it is common sense that is modified. If our conception of rationality leads us to the conclusion that all science is irrational metaphysics and should be abandoned, what we should do is adjust our notion of rationality to a more realistic level. Once a revolution in science has taken place, our ideas of rationality and common sense gradually change. When a radically new theory has been accepted, our ideas of what makes sense, or is reasonable, undergo significant shifts. Falsification can thus be said to have a historical character, and Lakatos agrees with Kuhn in holding that the history of science cannot be fully understood without mob psychology ([4]). Relatively few experiments are really important and which pattern is actually

([3]) POPPER, Karl, «Normal Science and its Dangers,» in Imre Lakatos and Allan Musgrave (eds.) *Criticism and the Growth of Knowledge,* Cambridge University Press 1970, p. 57.

([4]) LAKATOS, Imre, «Falsifation and the Methodology of Scientific Research Programs,» in Lakatos, *op. cit.,* p. 140, note.

realized depends often on historical accident. At best the rationality category is historical, applicable to science after the event when the long range consequences and developments are known. But once we acknowledge that no conceptual change is ever absolute, we are left with the consequence of greater and lesser conceptual modifications differing from one another in degree.

Feyerabend denies that there is any uniform «method» in such change but admits that the attempt to create knowledge needs guidance. The guidance that knowledge requires is not a rule, it is rather a theory, a point of view that allows the researcher to separate the relevant from the irrelevant. There no longer exists a single set of rules that will guide us through all the facts and turns of the history of science. But while rejecting «method,» Feyerabend takes progress in science for granted: progress consists in increasing our power, in being able to do what we want to do. Still, Feyerabend is not a pragmatist; the absurd theory may win the day and the plausible theory may have to be given up — this is what happened at the time of Galileo. Modern physics started not as an observational enterprise but as an unsupported speculation that was inconsistent with highly confirmed laws. Crucial theories work well if theories are compared with respect to a much more general background theory which provides a stable meaning for observation sentences. Strict adherence to meaning invariance would made have impossible some very decisive advances in physical theory. «Progress» in science can be maintained only on the background of what such diverse theories have in common. The structure of scientific revolutions actually exhibits a *partial* invariance and translability as Kuhn himself came to admit There is no all-over progress in science even though there may be evolution in the Darwinian sense.

Kuhn has given us a new metascientific conception: the logical method must be replaced by the historical method. The particular kind of rationality which the philosopher of science imputed to science simply does not exist. Kuhn consequently cannot give any sense to the notion of scientific progress; in particular, the conviction that scientific development is a

growth process (5). Since different paradigms solve in fact different problems, it inevitably comes to a squabble over which problems and solutions are more important. This explains the circular nature of argumentation in paradigm debates and its purely persuasive character. A new scientific truth does not usually establish itself by convincing its opponents who then declare themselves enlightened, but rather because its opponents gradually die off and the new generation is familiar with the truth from the beginning. For the members of the victorious party the outcome of the revolution *is* progress. This group is also in the favorable position of being able to make sure that future members of the scientific community see things its way. What rationality is — if anything — under such circumstances is of course the issue. Kuhn is not able to indicate where the line between a tolerable and an intolerable level of anomalies lies and what causes thus a revolution in science. If paradigms are incompatible, then it remains unclear how they can be thought to compete with each other, to be rival alternatives. The question whether a theory can be rescued with the help of suitable auxiliary hypotheses or other immunization strategies is a problem for scientific morals. But it is not the task of the philosopher to legislate scientific practice. Theories are as a rule introduced not independently of each other but in a certain order, even though the normal progress of an empirical science is neither linear nor necessarily accumulative. Empirical sciences develop in a way that is different from mathematical disciplines: confirmation, falsification, and the dislodging of theories have no logical, mathematical analogue.

Most empirical accounts of science have been based, usually tacitly, on the notion of a comparatively unproblematic observation language. We no longer claim that the truth of such statements can be known incorrigibly, but the consequences of this significant admission of fallibility of even observation statements has not been generally recognized. Quine has shown that no descriptive statement can be individually falsified by

(5) STEGMÜLLER, Wolfgang, *The Structure and Dynamics of Theories*, Springer Verlag, New York, Heidelberg, Berlin 1976, p. 138.

evidence since adjustments in the rest of the system can always be devised to prevent its falsification. Hence explanation of observation by theory, or reduction of one theory to another, cannot take place by identification of the concepts of one theory with those of observations with another theory, nor by empirically established relations between them. We cannot even know that different theories are «about» the same observational subject matter. Translated into terms of human language users, the network model requires only that by learning to apply predicates in an intersubjectively acceptable manner, they have acquired physical dispositions which are invariant to change of evidence. It seems fairly clear from the history of science however that evidence also educates our dispositions. The solution of the meaning variance paradoxes requires that there are always many stable predicates when one theory gives way to another ([6]). There is no need to make a fundamental epistemological distinction between the theoretical and observational aspects of science.

## II

Quine observes that when predictions are falsified, the choice of what should be retained is made according to a «vague scheme of priorities.» One can indeed retain any sentence if one is prepared to make sufficient changes in the system. Although theories with different cores cannot be compared at the object level, they can be compared at the metalevel of theoretical discourse ([7]). The most significant aspect of Darwin's theory was his entirely novel insistence that evolution could not be interpreted as a goal directed process in which successive stages of development represent a more perfect realization from the very beginning ([8]). Kuhn wants to apply

([6]) HESSE, Mary, «Duhem, Quine, and a New Empiricism,» in Morick, *op. cit.*, p. 224-225.

([7]) STEGMÜLLER, *op. cit.*, p. 245.

([8]) KUHN, *op.cit.*, p. 171-172.

this aspect of Darwinian theory to the development of the natural sciences. The evolution of science, or scientific progress, should no longer be interpreted as a goal oriented process, as a development leading to the complete, objective, only true interpretation and explanation of nature. Popper's model may be appropriate only for the more primitive stages of thought where there is no need to distinguish between hypotheses and theories. What initially looks like degeneration can subsequently turn out to be progressive. We cannot demarcate science from all other intellectual activity and define the essence of science, as it were. The positivists are not agreed on what exactly the correct formulation of the universal criterion of science is; rather what they are agreed on is that the criterion is universal. As against this, Lakatos, Feyerabend, and Kuhn may be thought to say that rationality equals coherence within a social, contextual or intellectual framework. Kuhn especially has emphasized how a scientist depends on his society and how a realistic epistemology must be sociological rather than logical or psychological. Each culture has different criteria of reasonability based on our innate seeking for the more stable in our environment. To combat subjectivism Poppernian verisimilitude suggests that science has as its goal a total true description of the universe. But progress is a slippery notion in the context of an evolutionary theory save in the sense of short range adaption to the environment. We have here an account of change and of adaption out not necessarily an account of progress toward a definable goal. The idea that there is a ground plan for scientific progress then goes the way of the idea that a creator is guiding the direction of organic evolution (9). It is also possible that the laws invoked here are too complex to be grasped by human intelligence or that the syntactical structure of human language renders the appropriate statement impossible.

Quine demands that truth should hinge on reality, not lan-

(9) ACKERMANN, Robert John, *The Philosophy of Karl Popper*, University of Massachusetts Press, Amherst 1976, p. 92.

guage ([10]), but we are in no position to eliminate language in stating what the truth is. Quine admits that since data are supposed to be hard and incontestable, it is ironical that the very notion of datum should be shaky and controversial. He argues that the dilemma is resolved when we give up the dream of a first philosophy («rationality») firmer than science. Observation sentences at their strictest are sentences that are learned to use by direct conditioning to socially shared concurrent stimulation. What makes them so decisive in adjucating scientific theories is that all speakers of the language who are present and attentive when an observation sentence is affirmed are apt to agree in assenting to it or to agree in dissenting from it. Observation sentences are crucial in conveying of evidence and in the learning of language. The criticism against the notion of observation is that observation varies with the observer's interest and thinking. But Quine holds that this variation is arrested when we define observation sentence by reference to the linguistic community. The quality of being an observation sentence admits of degrees. The more observational a sentence is the more nearly can its use be mastered ostensively.

Goodman on the other hand argues that similarity is a relative variable, culture dependent. We must have at least a partial theory before we know we have a repetition of the experiment. Circumstances alter similarities ([11]). Systems of measurement tend to govern ordinary judgments at least as much as to be governed by them. Quine and Goodman each express doubts concerning synonymy with respect to both intra and inter-language translations ([12]). Radical meaning variance theorists could be construed as taking seriously such doubts about the possibility of exact synonymy. More importantly,

([10]) QUINE, W. V., *Philosophy of Logic,* Prentice Hall, Englewood Cliffs, New Jersey 1970, p. 4.

([11]) GOODMAN, Nelson, «Seven Strictures on Similarity,» in Lawrence Foster and J. W. Swanson (eds.), *Experience and Theory,* The University of Massachusetts Press 1970, p. 28.

([12]) QUINE, W. V., *From a Logical Point of View,* Harvard University Press, 1953, p. 63; Goodman, N., «On the Likeness of Meaning in Semantics and the Philosophy of Language,» L. Linsky (ed.), University of Illinois Press, Urbana, 1952, p. 73-74.

they could be construed as extending those doubts to cover the possibility of two terms being commensurable, similar or alike in meaning to *any* degree when they occur in different theories. Only by claiming that meanings can be similar and comparable in some respects do we make classical and relativistic mechanics comparable. Justification of scientific change rests thus on different sorts of invariance. The claim that there are pervasive presuppositions fundamental to scientific change seems to be essential to the views of Feyerabend and Kuhn. They claim that what is perceived in effect depends on what is believed, that men who accept different «ideas» or «paradigms» will see different phenomena. They think that previous thinkers «live» in an observational world very different from our own. One would have to maintain that a geocentric world picture isn't wrong after all. While Quine argues that nothing is analytically true, and that all truth and falsity is synthetic, Feyerabend maintains that nothing is synthetically true, that all truth and falsity is linguistic, and that the given is a myth. However, the fact that what is observed is influenced by belief does not imply that what is observed cannot be shared by holders of different beliefs. It is a historical truism that revolutions change much less than they seem at the surface. In a conflict between competing scientific theories there are normally many rather basic principes held in common by advocates of competing views. Paradigm shifts are due to change in belief, not vision, and men have more control over what they believe than over what they see. This is where Kuhn's analogy between science and politics is misleading. Essential revisions of belief about experience could not be made if Feyerabend and Kuhn were right. Progress in science would be an illusion. What a scientist ends up with *partly* depends on what he is looking for — but only partly. A gestalt switch in science not only can be rationally justified but invariably has to be.

If radical meaning variance were true, no theory could contradict or be consistent with another ([13]). But lack of isomor-

([13]) KORDIG, Carl R., *The Justification of Scientific Change,* Reidel Dordrecht-Holland/Boston 1971, p. 52.

phism is not a sufficient condition for either incomparability or disagreement between theories. If the doctrine of radical meaning variance were true, each scientist would be effectively isolated within his own system of meanings. Kuhn maintains that competing paradigms are addressed to radically different problems ([14]), but if this is so then true competition between holders of different scientific theories becomes impossible. There would be no sense left to the notion of a rational progression of scientific viewpoints from age to age ([15]). No change in science could be judged as constituting progress and the choice between paradigms would become altogether arbitrary. Kuhn gives no reasons for accepting one paradigm as better or more acceptable than another. Scientific objectivity becomes a myth, yet some sort of objectivity is presupposed by any statement which purports to make a cognitive claim. Framework principles such as the continuity of nature or the assumption that material things do not cease to exist without physical cause, belong to what Wittgenstein calls a «system.» All testing, all confirmation and disconfirmation of a hypothesis takes place already within a system, and this system belongs to the nature of what we call an argument. A «system» provides the conditions within which we ask questions, carry out investigations, and make judgments. Hypotheses are put forth, and challenged *within* a system, as do verification, justification, and search of evidence. But we do not *decide* to accept framework propositions. The framework propositions that we accept, and grow into, are not idiosyncrasies but common ways of speaking and thinking that are pressed on us by our community. As a result of our scientific training, we have an aversion to statements, reports, declarations, or beliefs that are not based on grounds. But grounds must come to an end. *Within* the scientific language game there is justification and lack of justification, evidence and proof, mistakes and groundless opinions, good and bad reasoning, correct measurements and incorrect ones. Malcolm asserts that in this sense religion is

([14]) KUHN, *op. cit.*, p. 149.
([15]) KORDIG, *op. cit.*, p. 70.

groundless and so is chemistry ([16]). But induction is not just belief in the sense of religious belief, it is a precondition of rational knowledge. To claim that religion is one form of life and science another is to ignore essential differences. In spite of historical transformations, certain core statements of science are virtually universal and permanent. Even if ultimately science rests on some kind of unproven belief, science is more than just one myth among others — it is the most grounded belief that we have and open to continuous correction by at least partially independent observations. It is misleading to say that empirical science is a «system,» for the inspiration behind the idea that knowledge forms an ordered hierarchical system has been mathematical and logical. Most specific issues in the theory of knowledge can be conceived as problems about the epistemological priority of one class of statements to another.

## III

While conceptual change is a change in one's language, it is far from clear what a way of perceiving, experiencing, or thinking about the world is or what it is for different people to perceive experience, or think about the world in different terms. Yet vague expressions like these are frequently all we are offered in explanation of the «conceptual schemes» which supposedly are tied up in different languages. A language embodying an altogether different scheme must be an untranslatable one, but because of its untranslatability we could not be aware of the difference of conceptual scheme. Species of classification are neither mere reflections of the facts nor transparent patterns superimposed on them for convenience and communication. People with different schemes recognize different resemblances and can be expected to make different predictions and to offer different explanations. What counts as data is revised as a result of conceptual change, and shifts

([16]) MALCOLM, Norman, *Thought and Knowledge*, Cornell University Press, Ithaca & London 1977, p. 208-209.

as our assignments of subjective probability affect what it is reasonable to select as evidence. The distinction between framework and content may be drawn in different ways, depending on the purpose served by the distinction. Since a change in belief can affect a change in meaning, the analyticity of a sentence is not a sufficient ground for its acceptance. Questions of reference have no meaning except relative to some background language, yet there is no privileged background language as there is no privileged coordinate system. People who hold different sorts of preferred statements disagree about what they consider to be the most fundamental and important truths about the world. It is possible to think of core statements as analytic; while they are not declared true by definition, they are taken to be true unless a special case is made against them. Thus, although one may rationally embark upon work on a theory which contradicts the available background knowledge, Feyerabend's thesis that «anything goes» in science ignores the implicit structure and coherence of all knowledge expressed in language.

There is an indefinite number of degrees in the relative fixity of patterns. In all methodic activity antecedent factors are required, whether they are called expectations, strategems, or ideas. «Rational» has numerous meanings emerging from historical and analytical contexts that are difficult to interconnect. In subsequent historical estimate, of course, a deviant type of unity may come to be accepted. How «continuity» and «progress» are recognized depends on the way in which the concept of rule is interpreted. Our intuitions always carry us beyond our immediate vision, and we never fully know what we mean for we can never exhaust the perspective within which our judgments are made. Presupposed by the linkage between comparability and translatability is the assumption that every parameter relevant to the comparative appraisal of theories requires a translation of the object-level claims of one theory into the object-level language of another theory. Landau convincingly argues that there are many features relevant to their epistemic appraisal which can be determined without any machinery for translating the substantive claims

of the theory into any other language. The fact (if it is a fact) that scientists who subscribe to radically different theories about the natural world may not understand one another does not preclude the possibility that — if they share certain meta-level goals — they may nevertheless be able to agree on the relative cognitive success and failure of their respective theories ([17]). Incommensurability of theories on the object level does not entail incommensurability at the metalevel. There is a myth that scientific theories are like logical systems, particularly that they are like proofs in mathematics. But in actual science deductive systems are quite rare. Every theory of knowledge is itself reflected by the form which science takes at the time and from which it can obtain its conception of the nature of knowledge. The very principles in the light of which knowledge is to be criticized are themselves found to be socially and historically conditioned. New forms of knowledge grow out of the conditions of collective life and do not depend for their emergence upon the prior demonstration by a theory of knowledge, or by being «rational.» The relationship is actually quite the reverse: the revolutions in methodology and epistemology are always sequels of the revolutions in the immediate empirical procedures of getting knowledge.

Tarskian semantics give no explanation of the meanings of «true» and «false» when they are used to compare and criticize different theories, if meaning is really theory dependent. It is precisely the extratheoretical notions of truth and falsity that are indispensable for rational criticism, which is why they have always been taken as fundamental in logic. It often happens in a scientific revolution that something that was once taken to be an a priori truth is given up. Philosophers have consequently always found it necessary to draw upon the scientific knowledge of their time, and scientists have always found it essential to do a certain amount of philosophy in their very scientific work, even if they denied that that was what they were doing. Scientific method rests, beyond a certain

([17]) LANDAU, Larry, «Two Dogmas of Methodology,» *Philosophy of Science*, vol. 43, Number 4, December 1976, p. 594.

point, on a *de facto* agreement as to what is «plausible» and what is «ad hoc» (or just silly) ([18]). In any rational way of life there must be certain arbitrary elements. Analytical statements are statements which we all accept and for which we do not give reasons. The problem is to distinguish them from other statements which we unreasonably accept. The search for a line of demarcation is based on the assumption that it is possible to discover criteria of scientific states which do not change with time. The need to deliminate science springs mainly from proliferation of doctrines which pretend to be scientific but which do not satisfy even the most elementary methodological criteria accepted by scientific communities. The criterion of demarcation, regardless of how we formulate it, is normative in character, and so is the notion of progress. The acceptance or rejection of such criteria is always a matter of convention. Any rational discussion of such norms can only pertain to their usefulness, and this is possible only under the condition that there is agreement as to the ends which the accepted norm is to serve. Whenever we state that a certain theory is or is not scientific, whenever we express an opinion on when science was born, we do this on the ground of some conventionally accepted definition of science. By contrast, no criteria of demarcation or progress can be formulated on the grounds of a strict descriptive methodology of science. Such an approach can tell us only that science is what scientists are doing, and that scientists are men who are considered scientists in their epoch. Yet the historian of science (including Kuhn) cannot help making value judgments, by defining science, comparing different paradigms, and the like.

Such normative methodologies of knowledge development are never constructed in isolation from the actual history of science. The history of science is always written from the point of view of some philosophy that provides the definition of what science is. Only if the language of science were closed and coherent would a change in language always imply a change in

([18]) PUTNAM, Hilary, *Mind Language and Reality*, Cambridge University Press, 1975, p. 31.

our conceptual apparatus and world perspective. Since however the language which we utilize is not closed and coherent, a paradigm switch only leads to the same judgment being expressed in different terms and the new formulation is thus translatable into the old language. The gradual weakening of the theory-observation dichotomy is one of the most significant developments in the contemporary theory of knowledge. How we make sense of science depends on what else we know. Some form of framework (Kant's synthetic a priori or a more weakened form of some «language») is presupposed in all claims of rationality and progress in science. But to say that science is rational or progressive is more than just to claim that it can be formulated in a coherent fashion. It is to assume some correspondence to «reality.» The philosopher who is concerned with he rationality of science really wants to set science up as a guide of life. Rationality and progress are not something discovered about science but something postulated in advance. The asymmetry between the past and the future of scientific research has much greater methodological significance than the merely logical asymmetry between verification and falsification.