THE STRUCTURE AND DEVELOPMENT OF SCIENCE

Edited by

Gerard Radnitzky

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PRESUPPOSITIONS, PROBLEMS, PROGRESS

According to a wide-spread view science grows linearly and cumulatively. Brick is laid upon brick, and in this way the edifice of science is slowly but constantly growing. But studies of the history of science have shown that the development of science is far more dramatic. There are scientific revolutions which do not leave stone upon stone.[1] Thus the scientist is not only a master builder, but also a demolition expert; and science is growing like a modern town: old houses are destroyed and replaced by new ones.

A consequence of the new, historical way of regarding science is that we have fewer reasons to think that our knowledge eventually will be complete. If science is growing linearly and cumulatively you can ask if the growth of knowledge will reach a point where everything or almost everything is known, where the edifice of science is finished. Many philosophers and scientists have thought that scientific knowledge can be complete in this sense. In this volume Rescher cites the American philosopher Charles Sander Peirce (1839–1914), who thought that in his own day physics was almost complete, and that only some minute increases in its accuracy could still be achieved.

Rescher presents different arguments against the idea of complete knowledge. One argument is that when a scientific question is answered, the answer itself makes it possible to raise new questions, for example if a new hypothesis can be further explained. Rescher formulates a principle of question propagation saying that the solution of any scientific question gives rise to yet further unsolved questions.

It has been argued that there are ultimate answers that cannot be further questioned. Such an ultimate answer (or ultimate explanation) might for example describe the *essential* properties of things, which cannot be further explained. In modern philosophy of science this view, which Popper has called *essentialism*, has been given up.[2] Already Kant maintained that the 'real essence' or 'nature' of a thing was inaccessible to human knowledge. This is the basis of what Rescher calls the 'Kant Proliferation Effect', according to which every answer given on principles of experience begets a fresh question.

A kind of weak essentialism is involved when questions about theoretical or linguistic frameworks are declared 'improper' or 'illegitimate' or 'meaning-

G. Radnitzky and G. Andersson (eds.), The Structure and Development of Science, 3–15.

less'. Such a position implies that certain hypotheses or linguistic conventions simply have to be accepted without further question. But why should we accept certain theoretical or linguistic frameworks as essential in the sense that they cannot be further questioned? Is not the attempt to declare certain questions to be illegitimate or improper merely an expression of a hidden dogmatism hindering the growth of knowledge? In science at least we have no reason to expect that we will find ultimate explanations which cannot be further questioned. As Kuhn has pointed out, the history of science is characterized by scientific revolutions demolishing earlier theoretical frameworks. What earlier was thought to be essential and absolute has to be given up for new and hypothetical explanations. Rescher's investigation of the limits of science suggests that science is an unending quest.

Jammer argues that science has philosophical implications. The boundaries between science and philosophy are not so well-defined and distinct as an earlier positivist philosophy of science thought. In the history of science philosophy has given important ideas and inspiration to science, as Jammer has shown in his important studies about the role of such central concepts as space and force in the development of physics.[3]

But Jammer maintains not only that philosophy has heuristic value for science, but also that science rests on philosophical presuppositions. These presuppositions are not absolute or essential, but change in the course of and as a result of scientific research. An example would be the assumption that undisturbed experience is always reliable. This is a philosophical presupposition, a presupposition of naive realism. But scientific research has shown that the relation between theory and experience is much more complicated than assumed by naive realism. As a result this philosophical presupposition has had to be modified. Thus the internal development of science itself causes a change of philosophical presuppositions, and they come back in a modified form as philosophical implications of a scientific theory. This is the explanation of why science has philosophical implications. Jammer gives a number of examples of such developments from modern physics.

Earlier science had philosophical implications in a wider sense. The great scientific revolutions of the past destroyed philosophical world pictures and replaced them by new ones. This was for example the case with Aristotle's and Newton's physics. But from the recent scientific revolutions no intellectually satisfying world picture has emerged. In spite of the immense growth of knowledge modern man finds himself lost in a universe which he cannot understand. This is a new situation in the history of rational human thought. The lack of an intellectually satisfying world picture is in a sense a lack of philosophical implications of modern physics. In order to overcome this situation Jammer argues for a harmonious cooperation between science and philosophy, a cooperation in which physics gains its dignity by serving philosophy. In this way a kind of scientific culture can be maintained in spite of irresistibly increasing specialization. The future will show whether this cooperation can be achieved in a time when philosophy fears speculation and metaphysics, and science is bound to a narrow and utilitarian perspective on knowledge.

In his dialogue on method Feyerabend argues that science is only one of many different forms of life, and not necessarily the best one. Adherence to science, logic and rationalism is in reality only a more or less dogmatic adherence to a certain Western form of life with its origin in ancient Greek philosophy.

Already some Greek philosophers were against myth and poetry. As is well known, Plato wanted to ban poets from his ideal republic. Feyerabend suspects that modern 'rational' philosophers have a similar negative attitude towards poetry and myth. But such an attitude could exclude us from many interesting forms of life. With a number of examples Feyerabend shows us what myth and art can teach us. We are invited to listen to the verses of romantic poets, to the ecstatic experiences of mystics, and to take a look in the kitchen of a witch and in the hut of a medicine man, where interesting things unknown to science are going on.

Basically Feyerabend is arguing for openness towards other forms of life. Earlier he has argued for theory pluralism, and now he is arguing for a plurality of forms of life, because he thinks that openness to other forms of life, readiness to learn from them, is desirable and even a sort of rationalism. We should be like children, open to reality and ready to learn. This is the positive side of Feyerabend's teaching. In a time with much blind belief in science this is important. What J. S. Mill said about the Socratic dialectics can perhaps also be said about Feyerabend's dialectics:

> They were essentially a negative discussion of the great questions of philosophy and life, directed with consummate skill to the purpose of convincing any one who had merely adopted the commonplaces of received opinion, that he did not understand the subject . . . .[4]

Feyerabend is doing something similar in a time when the commonplaces of received opinion are partly offered by science. Therefore he fights the commonplaces of received opinion concerning witchcraft and astrology, and is a gadfly of dogmatic scientism. From a criticist point of view Feyerabend's 'negative discussion' is to be welcomed. J. S. Mill said that

> ... if there are any persons who contest a received opinion, ... let us thank them for it, open our minds to listen to them, and rejoice that there is some one to do for us what we otherwise ought, if we have any regard for either the certainty or the vitality of our convictions, to do with much greater labour for ourselves.[5]

This of course applies particularly to persons like me who are not particularly attracted either by witchcraft or by astrology.

According to J. S. Mill, Socratic dialectics and his negative discussions aimed at clearing the way for a well-grounded judgment. Nothing similar can be found in Feyerabend. In this respect his dialectics are more similar to the Sophists' than to Socrates'. Also in his attitude to and use of rhetoric. Feyerabend is much closer to the Sophists than to Socrates. Concerning the choice of a form of life or scientific theory Feyerabend does not give any guidelines, but says that it is a result of propaganda, that is, of the modern form of rhetoric. Thus not only does Feyerabend tell us to be open to different forms of life, but in the end he hardly can avoid the conclusion that all forms of life are equally good from a theoretical point of view. In this way Feyerabend's philosophy ends in scepticism. We are told to be open to other forms of life, but it is the openness fostered by relativism and scepticism. Is there not a risk that the end result of this philosophy is someone dangling between all forms of life, mixing black magic and particle physics, astrology and positivist philosophy? Such relativism as the result of a scientific culture was predicted by Nietzsche a long time ago[6], and has been described by Musil in his novel *Der Mann ohne Eigenschaften*.

Hodgson argues that science necessarily has presuppositions and limits. That science has limits does not imply that scientific knowledge will be complete, that science can solve all its problems. Hodgson's position does not contradict Rescher's idea of the propagation of questions, i.e., that every answered question begets a new question. Instead, Hodgson argues that science cannot solve all kinds of problems. It cannot tell us what is good or beautiful, and thus cannot solve esthetic and ethic problems, which perhaps are at least as important as the problem of giving scientific explanations and predictions of phenomena.

There have been attempts to derive or extrapolate morals from science itself. Hodgson does not think that they have been successful. They presuppose that we first accept the supreme value of science, which is a value judgment lying outside the domain of science. Hence Hodgson concludes that the whole area of moral judgments lies outside science.

Hodgson thinks that science cannot even begin if the prospective scientist does not believe that the world is ordered and rational. Otherwise he would think that the scientific enterprise is doomed to failure. In this sense certain metaphysical presuppositions are necessary for the very beginning of science.

According to Hodgson this can explain why science was born in Europe and not elsewhere. The Christian belief in a rational and omnipotent Creator firmly implanted in the European mind the conviction that the universe is ordered and rational and opened the way for science.

But Hodgson also remarks that it is common for any ideology to propose its own belief as the necessary basis for science. This can have the effect that scientific results which do not fit with the allegedly scientific ideology are not accepted and even that whole areas of science are destroyed for ideological reasons. As the history of our century teaches us, a 'scientific' ideology can be a serious hindrance to science.

It is interesting to note that most ideologies today, irrespective of their origins, maintain that they are either based on science or even the basis of science itself. In this way they try to find a 'scientific' legitimation. Science has taken the place previously occupied by divine revelation or reason. This shows how victorious science has been in our culture. But according to Hodgson's investigations of the necessary limits of science, science cannot fulfil such a task. The claim that an ideology is 'scientific' can only be a rhetorical one.

Heelan argues against the idea of the linear growth of science. As can be seen in the history of science, a scientific theory can be developed in more than one direction. We get what Stegmüller has called 'branching off' in his contribution: from one theory many theories can develop simultaneously. If the developed theories do not contradict each other, there is always a possibility that they can be unified in a higher synthesis. This is the lattice model for the growth of science in contradistinction to the linear growth model. Heelan thinks that the lattice model is more realistic than the older linear model.

In the philosophy of science there are formal and historical approaches. The philosophy of science inspired by logical empiricism analyzed scientific theories logically. This approach was inspired by metamathematics. As a result scientific theories were regarded as formalisms with a partial interpretation. Inspired by David Hilberts's axiomatization of geometry, Moritz Schlick (in *Allgemeine Erkenntnislehre*) had championed the doctrine that the basic concepts of a theory are only 'implicitly defined' by the axioms of the theory. These ideas were later developed by Norman R. Campbell, R. Carnap and others. This view has been called the 'orthodox' view (Feigl) or the 'standard conception' (Hempel) of scientific theories.[7] It basically rests on an analogy between mathematical and scientific theories: scientific theories are regarded as partially interpreted formal systems. This approach allows us to use methods developed in metamathematics and in formal logic in order to analyze science. Earlier propositional and first order logic were the main tools for these investigations. Recently P. Suppes has enriched the formal tools with set theory. Theories are formally investigated and analyzed by defining different set-theoretical predicates. This is the method used by Stegmüller in this volume.

There has been opposition against the dominance of logical methods in the philosophy of science: it was argued that in order to understand the development of science historical investigations are necessary, that logical analysis alone cannot grasp the dynamics of theories. As a result some philosophers of science wanted a new and more historically oriented philosophy of science. This opposition was mainly directed against the logical empiricist philosophy of science.

An important advocate of a historically oriented philosophy of science is Thomas Kuhn. He maintains that not only positivism but also falsificationism were inadequate for understanding the development of science. Kuhn's main argument against falsificationism is that falsification does not at all play the role in the development of science that falsificationism has maintained. As a matter of fact the history of science shows us that most theories are falsified but nevertheless accepted by the scientific community.[8]

In his paper Stegmüller accepts the criticism of falsificationism presented by Kuhn, Lakatos and others. But he does not think that the criticism of the formal approach from the history of science is fatal, on the contrary, he tries to show that the *formal* and *historical* approaches can be *combined*. This explains the title of Stegmüller's contribution: 'A combined approach to the dynamics of theories'.

The traditional formal approach in the philosophy of science has been modified in important respects by Sneed. Sneed tries to meet some criticisms of the 'partial interpretation' view. Important in this criticism were the charges that the distinction between observational and theoretical concepts is not sharp or fundamental and secondly that there are no observation statements that are free of theoretical presuppositions.[9] In order to solve these problems Sneed gives up the idea of a purely observational language, and develops new methods for determining which concepts are theoretical in a theory and what empirical content a theory has.

As a result of these modifications of the standard view, Stegmüller no longer speaks of partial interpretation of a theory, but of its *intended applications*. Following Sneed, Stegmüller has argued that in reality physicists only have definite intended applications of their theories in mind, and hence that the idea of the strictly universal application or validity of theories belongs to the 'metascience of science fiction'.[10] Theories are no longer regarded as formalism with partial interpretation, but as formalism with intended applications. This is the basis for Stegmüller's 'non-statement' or 'structuralist' view of theories.

Of course a formalism or a 'non-statement' cannot be falsified. The intended applications of the formalism can be successful or not. This is different from a test of the truth values of statements. According to the 'non-statement' view it does not make sense to speak about the truth-value of a theory. You can only say that a theory is more or less successful in intended applications. At this point Stegmüller thinks that the historical and formalist schools in the philosophy of science meet each other. Kuhn and Lakatos maintained as a result of historical studies that the basic structure of a theory or a research programme, what Lakatos called hard core and Kuhn paradigm, as a matter of fact is not falsified in normal research. The formalist school says that the basic formal structure of a theory cannot be falsified. Thus both schools agree on the unimportance of falsifications. Furthermore both schools agree that for the acceptance of a theory successful applications are basic.

Encouraged by these similitarities Stegmüller thinks that the historical and formalist school should join their efforts in order to understand the historical change of scientific theories and erect a stable bridge of cooperation between each other. At the same time he rejects falsificationism as ahistorical and beyond repair.

Against the claim that falsifications are unimportant for the development of science, a falsificationist would answer that the refusal to take falsifications seriously in reality is a refusal to learn from experience. He could say that you can always 'immunize' your theories from cirticism but that such a strategy does not lead to growth of knowledge.

An adherent of the structuralist view of theories probably would answer that a falsificationist has not understood what a theory really is, namely a formalism with intendended applications. Therefore he places unrealistic demands on science. He is not, as he himself would like to think, a critical rationalist, but an uncritical dreamer.[11]

On the other hand, a philosopher close to falsificationism has judged earlier verions of the 'non-statement view' in the following way: "Some philosophers, perceiving that it is not enough to take just the theoretical content out of scientific theories, have resorted to the desperate step of taking all the factual content out of them".[12] In this introduction I can only point at the violent clash between the falsificationist and formalist approaches: falsificationists regard the non-statement view as a desperate step, and formalists regard falsificationists as eccentric, unrealistic, and ahistorical.

Basically, the structuralist and the falsificationist view theories in different ways. For a falsificationist a theory consists of hypothetical statements which are true or false, and which can be tested by experience. For a structuralist a theory consists of a formalism which can be more or less successfully applied, but there is no direct link between theory and experience. "In the picturesque but illuminating elucidations used, e.g., by Schlick, Carnap, Hempel, and Margenau, the 'pure calculus', i.e., the uninterpreted postulate system, 'floats' or 'hovers' freely above the plane of empirical facts".[13] Only through the 'connecting links' of 'intended applications' is it related to experience. In short, a falsificationist adheres to theory realism, and a structuralist to theory instrumentalism. Thus there are very profound philosophical differences between them, and this explains their violent disagreement.

Kockelmans says that history of science and logic are necessary in order to understand science and its development, but that they are not enough. In order really to understand science phenomenological (or epistemological) investigations are necessary. Thus Kockelmans argues for a deepening of Stegmüller's combined approach with the help of phenomenology. Kockelmans says that he does not disagree with Stegmüller, but that he wants to go further.

Central for this approach is the question concerning the *telos* of that complex form of intentional behavior of man that constitutes scientific insights. Science has certain conditions of possibility which logic of science must presuppose, but cannot itself analyze. These conditions are connected with the genuine meaning of the scientific thematization in each science, with the conceptions of rationality and truth, and the goal of the scientific enterprise. With phenomenology these preconditions can be studied, and critically justified.

There are different possible orientations toward the world. Science is only one of them. Kockelmans holds that with phenomenology the very foundations of such orientations can be analyzed, and that only through such studies can a real understanding of science and of man's theoretical attitude toward the world be achieved.[14]

Radnitzky argues against the idea that the acceptance of a theory can be *justified*. Two problems are involved here. (1) Is there any secure empirical foundation of science? Supposing that there is such an empirical foundation we get the second problem: (2) can the acceptance of general scientific hypotheses be justified with reference to the empirical basis? We are confronted with the problem of the empirical basis and the problem of induction.

Following Popper, Radnitzky denies that there is any secure empirical basis for science.[15] Observation sentences are fallible and theory dependent. This implies not only a farewell to observation language, but also a farewell to the idea that science rests a secure basis of experience.

Following Hume and Popper, Radnitzky denies that the problem of justifying theory choice can be solved by any principle of induction or by any variant of inductive logic. If we are consistent followers of the philosophy of justification, we also have to give a justification of induction itself. But such as attempt leads to insurmountable difficulties.[16]

Radnitzky suggest that instead of trying to justify theory choice, we should try to give good reasons for the preference of a theory. Philosophy of science should give up the unrealistic ideal of justification for the more realistic ideal of rational preference. This problem shift constitutes a 'big divide in the philosophy of science'.

According to the new approach we can give up the old quest for certainty or for a secure foundation without being delivered to scepticism. Through criticism theories which are falsified by experience can be eliminated and better ones substituted for them. In this way we can hope to come closer to the truth, to get growth of knowledge.

It might be interesting to compare these results with Stegmüller's. In this volume Stegmüller says that at least in our world of limited lifetime and scare resources theory choice ceases to be the object of theoretical justification and becomes a case for rational decision theory. Referring to Wittgenstein's posthumous book *On Certainty* he says that there exists no particular, simple criterion of progress, but that there only can be *good reasons* for theory change. These reasons are numerous and very from case to case. Thus both Radnitzky and Stegmüller deny that we can give a theoretical justification of theory choice, and say that there can only be good reasons for it.

Which good reasons does Radnitzky propose for theory choice? If we compare two theories which have not been tested, we should prefer the theory with most empirical content. Here empirical content is not used in the traditional sense of content which can be translated into some empirical or observational language, but in the Popperian sense according to which the empirical content of a theory is proportional to how falsifiable it is. Our first, pre-testing criterion of preference thus is that we should prefer a more falsifiable theory to a less falsifiable.

After empirical tests we should prefer the theory which has withstood the most severe tests, that is, the theory with the highest degree of corroboration. What is a severe test? A test is severe in those areas where the tested theory is at variance with the background knowledge. Radnitzky discusses various definitions of background knowledge,[17] and comes to the conclusion that degree of corroboration cannot be our sole good reason for theory choice. Radnitzky also examines Popper's ideas about verisimilitude. He thinks that it would be better to speak about achievement or strength as representation. But the idea of verisimilitude only pushes our original problem of theory change one step back. The original problem reappears as the question of what good reasons we can have for thinking that one theory is closer to the truth than another. Radnitzky tries to answer this question in Section 2.2, 'Prediction and explanation as trials for theories'. His conclusion is that the attempts to establish 'a single cumulative index' based on earlier predictive and explanatory successes and failures is too ambitious. For theory choice we also have to consider the importance of the problems a theory tries to solve, and thus we are eventually led to an evaluation of the scientific importance of a question. In the last section of his paper Radnitzky tries to give an objective explication of what can be meant by 'scientific importance of questions'.

In summary we can say that Radnitzky thinks that there are a plurality of good reasons for theory choice. By choice between two theories we should consider (before testing) their falsifiability, and (after testing) how severely they have been tested (degree of corroboration), their predicitive and explanatory successes and failures (as a fallible indicator for verisimilitude), and the scientific importance of the problems they try to solve.

The aim of Howson's paper is to investigate whether Lakatos's methodology of scientific research programmes (MSRP) can be used for appraisal of non-empirical disciplines like pure mathematics. In the empirical sciences Lakatos distinguishes between progressive and degenerating research programmes. Roughly speaking, a research programme is progressive if independently of other research programmes it leads to new predictions at least some of which turn out to be correct. Otherwise it is degenerating.[18]

According to Howson we can find inductivism and hypotheticodeductivism not only in empirical sciences, but also in mathematics. Inductivism says that mathematical axioms or scientific laws can be found by simple generalization from what is immediately given, in science immediately given by experience. But what is immediately given in mathematics? Our intuitions about properties of mathematical entities. Having to choose between empiricism and intuitionism, Howson argues that sentences about mathematical intuition and 'protocol sentences' in science are very much on a par epistemologically. Thus there is an intuitive basis in mathematics, corresponding to the empirical basis in science.

Now Howson rejects inductivism and argues for hypotheticodeductivism in science and in mathematics. This mean that a theory or axiom system is appraised with reference to its deductive consequences. The more the deductive consequences of a theory or axiom system cover our intuitive or empirical basis, the better it is. In this way we can appraise mathematical research programmes with reference to an intuitive basis in a way similar to that in which scientific research programmes are appraised with reference to an empirical basis.

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NOTES

[1] Kuhn (1962).
[2] Popper (1963), Ch. 3.
[3] Jammer (1954), (1957).
[4] Mill (1859), p. 55.
[5] Mill (1859), pp. 56f.
[6] Nietzsche (1892), Part II, in 'Vom Lande der Bildung'. There Nietzsche writes:

> Alle Zeiten und Völker blicken bunt aus eueren Schleiern; alle Sitten und Glauben reden bunt aus eueren Gebärden.
>
> . . . .
>
> Alle Zeiten schwätzen wider einander in eueren Geistern: und aller Zeiten Träume und Geschwätz waren wirklicher noch, als euer Wachsein ist!

[7] See Feigl (1970) and Hempel (1970).
[8] Kuhn (1962).
[9] Feigl (1970), p. 8.
[10] Stegmüller (1973), p. 78.

[11] Vgl. Stegmüller (1973), p. 23, where he speaks about 'überspannter Rationalismus'.
[12] Watkins (1978), p. 30.
[13] Feigl (1970), p. 5.
[14] Cf. Kockelmans (1966) and Brand (1971).
[15] Popper (1959), Ch. 5.
[16] Popper (1959), Ch. 1.
[17] Cf. Watkins (1978), Worrall (1978), Zahar (1978), and Musgrave (1978).
[18] Cf. Lakatos (1970).

BIBLIOGRAPHY

Brand, G.: (1971), *Die Lebenswelt: Eine Philosophie des konkreten Apriori*, de Gruyter, Berlin.

Feigl, H.: (1970), 'The "Orthodox" View of Theories: Remarks in Defense As Well As Critique', in Radner and Winokur, 1970, pp. 3–16.

Hempel, C.: (1970), 'On the "Standard Conception" of Scientific Theories', in Radner and Winokur, 1970, pp. 142–163.

Jammer, M.: (1954), *Concepts of Space*, Harvard University Press, Cambridge, Mass.

Jammer, M.: (1957), *Concepts of Force: A Study in the Foundations of Dynamics*, Harvard University Press, Cambridge, Mass.

Kockelmans, J. J.: (1966), *Phenomenology and Physical Science: An Introduction to the Philosophy of Physical Science*, Duquesne University Press, Pittsburg.

Kuhn, T. S.: (1962), *The Structure of Scientific Revolutions*, Chicago University Press, Chicago.

Lakatos, I.: (1970), 'Falsification and the Methodology of Scientific Research Programmes', in Lakatos and Musgrave, 1970, pp. 91–195.

Lakatos, I. and Musgrave, A. (eds.): (1970), *Criticism and the Growth of Knowledge*, Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Vol. 4, Cambridge University Press, London.

Mill, J. S.: (1859), *On Liberty*, Reprinted in *On Liberty, Representational Government, The Subjection of Women: Three Essays*, The World's Classics, Oxford University Press, London 1912.

Musgrave, A.: (1978), 'Evidential Support, Falsification, Heuristics, and Anarchism', in Radnitzky and Andersson, 1978, pp. 181–201.

Nietzsche, F.: (1892), *Also sprach Zarathustra*, Kröner Taschenbuch 75, Alfred Kröner Verlag, Stuttgart, 1969.

Popper, K. R.: (1934), *Logik der Forschung*, Julius Springer, Wien (mit der Jahresangabe 1935).

Popper, K. R.: (1959), *The Logic of Scientific Discovery*, translation of Popper, 1934, with new appendices. Hutchinson and Co., London.

Popper, K. R.: (1963), *Conjectures and Refutations: The Growth of Scientific Knowledge*, 2nd revised ed., Routledge and Kegan Paul, London, 1965.

Radner, M. and Winokur, S. (eds.): (1970), *Analyses of Theories and Methods of Physics and Psychology*, Minnesota Studies in the Philosophy of Science, Vol. 4, University of Minnesota Press, Minneapolis.

Radnitzky, G. and Andersson, G. (eds.): (1978), *Progress and Rationality in Science*, Boston Studies in the Philosophy of Science, Vol. 58, Reidel Publ. Co., Dordrecht, Holland.

Stegmüller, W.: (1973), *Probleme und Resultate der Wissenschaftstheorie und Analytischen Philosophie*, Band 2: *Theorie und Erfahrung*, Studienausgabe, Teil D: *Logische Analyse der Struktur ausgereifter physikalischer Theorien, 'Non-statement view' von Theorien*,' Springer, Berlin.

Watkins, J.: (1978), 'The Popperian Approach to Scientific Knowledge', in Radnitzky and Andersson, 1978, pp. 23–43.

Worrall, J.: (1978), 'The Ways in Which the Methodology of Scientific Research Programmes Improves on Popper's Methodology', in Radnitzky and Andersson, 1978, pp. 45–70.

Zahar, E.: (1978), ' "Crucial" Experiments: A Case Study', in Radnitzky and Andersson, 1978, pp. 71–97.

NICHOLAS RESCHER

SOME ISSUES REGARDING THE COMPLETENESS OF SCIENCE AND THE LIMITS OF SCIENTIFIC KNOWLEDGE

1. KNOWLEDGE AND ITS DYNAMICS: A CRITIQUE OF THE GEOGRAPHIC EXPLORATION MODEL

The incompleteness of knowledge is a familiar conception, but nevertheless deserves closer scrutiny. A body of knowledge is *incomplete* when it is defective in point of comprehensiveness, self-sufficiency, and scope. In such a case, the body will have gaps, omissions, or such-like deficiencies in the inclusiveness of its 'coverage'. The whole is then lacking in mutual support – like a broken statue with missing pieces. Some parts of it will demand the operation of elements that are simply lacking, there is a collective inadequacy to the common task.

To probe more deeply into the workings of this idea of cognitive completeness in the context of our factual knowledge of the world it is useful to introduce some formal machinery, as follows:

> K_t': the set of theses (propositions, contentions, claims) explicitly endorsed as warranted (correct, acceptable, true) according to the systematization of scientific information accepted (i.e., generally accepted) at the time t.

Thus K_t' is simply the 'state of "knowledge" (i.e., *putative* knowledge) at t', comprising the broad consensus of the scientific community of the day.

This family of actually endorsed theses can be broadened so as also to embrace its various inferential consequences, which may or may not be explicitly recognized and overtly admitted:

> K_t'': the set of theses that are 'consequences' of K_t' (in some appropriate sense of the term). This is the totality of implicit (potential) 'knowledge' (i.e., *putative* knowledge) at the time t. (Note that while the *actually* espoused theses of K_t' must always, in the very nature of the thing, be finite in number, this limitation is removed with the transition to K_t''.)

G. Radnitzky and G. Andersson (eds.), The Structure and Development of Science, 19–40.

For abbreviative convenience a helpful convention will be adopted. We shall use K to indicate indifference as between K' and K'', as long as the filling-in is done *uniformly* throughout a given thesis.

The ambiguity of $p \notin K_t$ should be noted. This could mean either (1) at t people simply don't yet know that p, their ignorance is such that the whole question of p vs. not-p has not yet occurred to them, or (2) p is actually blocked from K_t because $\sim p \in K_t$, that is, because its contradictory is in fact known: the question of p vs. not-p is a live issue and has been resolved in favor of not-p.

Since it is *putative* or *purported* 'knowledge' that is at issue – knowledge as it is claimed by imperfect men, and not the capital-K Knowledge set down in the book of some infallible angel – we shall *not* have a

Law of the Conservation of Knowledge

What is once 'known' always remains 'known':

$$(\forall t)(\forall t')(\forall p)([t < t' \,\&\, p \in K_t] \supset p \in K_{t'}).$$

Such a 'law' fails for two reasons: (1) 'knowledge' can be forgotten, as in fact much of Greek astronomy was lost in the 'Dark Ages'; and (2) 'knowledge' can be abandoned: the scientific community may no longer accept a once accepted thesis, and indeed such a thesis may even be replaced by its contradictory (as much of Galenic medicine is actually inconsistent with modern medicine). The progress of science not only exhibits *additions* but *subtractions* as well. Our 'knowledge' is not cumulative. The body of 'accepted scientific fact' can be a matter not merely of augmentation but also of replacement.

The ramifications of this view are worth exploring at greater length.

One acute contemporary analyst of physics moots the prospect of its ultimate completion in the following terms:

It is possible to think of fundamental physics as eventually becoming complete. There is only one universe to investigate, and physics, unlike mathematics, cannot be indefinitely spun out purely by inventions of the mind. The logical relation of physics to chemistry and the other sciences it underlies is such that physics should be the first chapter to be completed. No one can say exactly what completed should mean in that context, which may be sufficient evidence that the end is at least not imminent. But some sequence such as the following might be vaguely imagined: The nature of the elementary particles becomes known in self-evident totality, turning out by its very structure to preclude the existence of hidden features. Meanwhile, gravitation becomes well understood and its relation to the stronger forces elucidated. No mysteries remain in the hierarchy of forces, which stands revealed as the different aspects of one logically consistent pattern. In that imagined ideal state of knowledge, no conceivable experiment could give a surprising result. At least no experiment could that tested only fundamental physical laws. Some unsolved problems might remain in the domain earlier characterized as organized complexity, but these would become the responsibility of the biophysicist or the astrophysicist. Basic physics would be complete; not only that, it would be manifestly complete, rather like the present state of Euclidean geometry.[1]

Extended from physics to natural science in general, such a position views the realm of potential discovery as one of ultimately limited proportions.

A position of just this sort was maintained by the great American philosopher Charles Sanders Peirce (1839–1914). Peirce, in effect, saw the history of science as progressing through two stages: an initial or preliminary phase of groping for the general structure of the *qualitative* relations among scientific parameters, and a secondary phase of *quantitative* refinement – of filling in with increasing precision the exact values of parameters that figure in equations whose general configuration is determined in the initial phase. Once the first phase has been gotten over with – as Peirce believed to be the case in his own day, at any rate with regard to the physical sciences – ongoing scientific progress is just a matter of increasing detail and exactness, of determining the ever more minute decimal-place values of quantities whose approximate value is already well-established.[2]

We have here a metaphysical view of cognitive evolution according to which science will finally reach a condition of ultimate cumulativity – that science is evolving along a winding and circuitous route into a condition of eventual stability in point of thesis-retention:

Law of the Ultimate Conservation of Knowledge

$$(\exists t)(\forall t')(\forall p)([t' > t \,\&\, p \in K_t] \supset p \in K_{t'}).$$

Peirce held just this view that science will ultimately reach – or at any rate asymptotically approximate – such a conservationist state in which whatever is 'known' will always remain 'known', so that everything then 'known' always remains 'known' thereafter.

A very different but related idea is that of knowledge-completion (at t). This envisages a circumstance where everything ever 'known' is then 'known':

Law of the Completion of Knowledge (at t)

$$(\forall p)(\forall t')([t' > t \,\&\, p \in K_{t'}] \supset p \in K_{t'}).$$

The combination of these two may be characterized as knowledge *ossification*. Its definitive principle may be formulated as follows:

$$(\exists t)(\forall t')(\forall p)(t' > t \supset [p \in K_t \equiv p \in K_{t'}]).$$

This principle envisages the eventual arrival of a condition of cognitive stability: the evolution of science towards a totally fixed and unchanging are sufficiently restricted (or unimaginative) as regards the questions we raise, the completeness of our knowledge will reflect this restrictedness rather than its own adequacy.

This perspective has important implications for the issue of the completability of science. Conceivably, if improbably, science might reach a fortuitous equilibrium between problems and solutions. It could be completed in the *effective* sense – in providing an answer to every question one *can* ask in the then-existing (albeit still imperfect) state of knowledge, yet without thereby being completed in the *fundamental* sense of answering the questions that would arise if only one could probe nature just a bit more deeply (as, alas, one cannot). The *perceived* completeness of science may accordingly fail to betoken its *actual* completeness.

5. THE ISSUE OF UNANSWERABLE QUESTIONS: TWO VERY DIFFERENT SORTS OF COGNITIVE LIMITS

The preceding section explored the idea of question-answering *completeness*. The lines of thought at work with the correlative idea of *incompleteness* point towards the issue of cognitive limits.

This theme of *cognitive limits* relates to the prospects of the question-resolving capacity of our knowledge in the long run. Now one significant way in which the question-resolving capacity of our knowledge might be limited is by the weak limitation asserted in the following thesis:

Weak-Limitation (The Permanence of Unsolved Questions)

> There are *always*, at every stage,[11] questions to which no answer is in hand. At every stage of cognitive history there are then-unanswerable questions (which, however, may well be answerable at some later stage):
>
> $$(\forall t)(\exists a)(a \in Q_t \;\&\; \sim(\exists p)A_t pa).$$

This thesis maintains a permanence of cognitive limitation – that our knowledge is never at any stage completed, because unanswered questions always remain on the agenda.

Note that if Kant's Principle of Question Propagation were accepted (cf. Section 3 above), then this situation of the permanence of unsolved questions would be assured. For if every answer generates further questions, then we should never reach a position where all questions are answered. It should also be observed that this position is perfectly compatible with the circumstance that every question arising at any given stage can be answered (or dissolved) at some *subsequent* stage. Weak limitation envisages the immortality of questions, and not the existence of immortal questions (insolubilia).

A second way in which the question-resolving capacity of our knowledge may be limited can accordingly envisage the far more drastic situation:

Strong-Limitation (The Existence of Insolubilia)

> There will (as of some juncture) be then-posable questions which will *never* obtain an answer, identifiable questions whose resolution lies beyond the reach of science altogether, questions that are always on the agenda, yet never soluble:
>
> $$(\exists a)(\exists t)(\forall t')(t' > t \supset [a \in Q_{t'} \; \& \sim (\exists p) A_{t'} pa]).$$

This thesis has it that there are immortal problems, permanently unanswerable questions, in short, genuine *insolubilia*.

Limits or restrictive boundaries upon knowledge can thus function in two very different ways. Consider the analogy of a reference library of a rather unusual sort – one with an *infinite* number of volumes. Suppose, as a first possible case, that only some finite number of its shelves are accessible. Then we have the situation of what may be characterized as a *terminating* limit on the information to be obtained: since only finitely many volumes can be attained, the body of knowledge to be derived – however vast it may be – must in the end remain finite. An inquirer will, in principle, have to come to the end of the road as regards the knowledge he can eventually secure: although still drastically incomplete, it will be incapable of any extension.

By way of contrast, consider the case in which only the last volume on every shelf of the infinite library is inaccessible. Clearly this too is a circumstance of restrictive limits. But such an *excluding* limit on the information to be obtained is something very different from the preceding *terminating* limit. For despite the undoubted existence of a very real limitation, the prospects of further substantial advances in knowledge are now always open. An inquirer can evermore extend his information in any given subject-matter direction as far as he pleases.

The point at issue was already clearly put by Kant, who was prepared to grant the actuality of excluding limits while vehemently denying that of terminating limits.

> In mathematics and in natural philosophy, human reason admits of *limits* ('excluding limits') but not of *boundaries* ('terminating limits'), namely, it admits that something indeed lies without it, at which it can never arrive, but not that it will at any point find completion in its internal progress. The enlarging of our views in mathematics and the

possibility of new discoveries are infinite: and the same is the case with the discovery of new properties of nature, of new powers and laws, by continued experience and its rational combination .... Natural science will never reveal to us the internal constitution of things .... Nor does that science require this for its physical explanations.[12]

This Kantian distinction between *terminating and excluding limits* is crucially relevant to our discussion. For in the context of scientific progress we must carefully distinguish two very different questions:

(1) Can we always improve (more than marginally) on the body of scientific findings we already have in hand?

(2) Does anything within the realm of the potentially discoverable lie entirely beyond our grasp, in being outside the range of what is possible for us to realize?

The former question comes down to: Does science have terminating limits? The latter to: Does science have *any* limits, be they terminating or excluding?

The issue of the prospects of ongoing scientific progress relates only to question (1) – it pertains to the question of *terminating* limits, and leaves that of *excluding* limits aside. The existence of inaccessible phenomena (and thus of 'unattainable findings') accordingly has no decisive bearing on the prospects of unending progress. And so to maintain (as has been done) the essential limitlessness of science on the side of terminating limits – the feasibility of unending scientific progress – is not to deny the prospect of problems whose solution lies beyond the physical and/or economic limits of man's investigative capacities. The existence of actually unanswerable questions in science – problems whose solution lies forever on the inaccessible side of a technologically imposed data-barrier – would *not* mean an eventual end to scientific progress.

The distinction between the two types of limits thus carries the important lesson – already drawn by Kant – that to accept the idea that scientific knowledge is limited is *not* tantamount to accepting the idea that science is finite or completable. The existence of unsolvable questions in natural science – of genuine *insolubilia* – will emphatically *not* entail the consequence that our knowledge in this sphere must ultimately terminate at some dead-end, issuing in a 'completed' state of knowledge whose boundaries we can extend no further. Local limits to knowledge are emphatically compatible with global limitlessness. (Think of the ever more comprehensive exploration of a limitless flatland replete with high peaks that one simply cannot scale.)

6. INSOLUBILIA AND THE REYMOND/HAECKEL CONTROVERSY

Is it plausible to suppose that there are scientific insolubilia? Is there good reason to suppose science to be limited in the strong sense rather than in the weaker sense of an ever-renewed pool of unresolved questions?

In 1880 the German physiologist, philosopher, and historian of science Emil Du Bois-Reymond published a widely discussed lecture on *The Seven Riddles of the Universe* (*Die sieben Welträtsel*),[13] in which he maintained that some of the most fundamental problems regarding the workings of the world were insoluble. Reymond was a rigorous mechanist. On his view, nonmechanical modes of inquiry cannot produce adequate results, and the limit of our secure knowledge of the world is confined to the range where purely mechanical principles can be applied. As for all else, we not only *do not* have but *cannot* in principle obtain reliable knowledge. Under the banner of the slogan *ignoramus et ignorabimus* ('we *do not* know and *shall never* know'), Reymond maintained a sceptically agnostic position with respect to basic issues in physics (the nature of matter and of force, and the ultimate source of motion) and psychology (the origin of sensation and of consciousness). These issues are simply *insolubilia* which transcend man's scientific capabilities. Certain fundamental biological problems he regarded as unsolved, but perhaps in principle soluble (though very difficult); the origin of life, the adaptiveness of organisms, and the development of language and reason. And as regards the seventh riddle – the problem of freedom of the will – he was undecided.

The position of Du Bois-Reymond was soon and sharply contested by the zoologist Ernst Haeckel in a book *Die Welträtsel* published in 1889,[14] which soon attained a great popularity. Far from being intractable or even insoluble – so Haeckel maintained – the riddles of Du Bois-Reymond had all virtually been solved. Dismissing the problem of free-will as a pseudo-problem – since free will "is a pure dogma [which] rests on mere illusion and in reality does not exist at all" – Haeckel turned with relish to the remaining riddles. Problems of the origin of life, of sensation, and of consciousness Heackel regarded as solved – or solvable – by appeal to the theory of evolution. Questions of the nature of matter and force, he regarded as solved by modern physics except for one residue: the problem (perhaps less scientific than metaphysical) of the ultimate origin of matter and its laws. This 'problem of substance' was the only remaining riddle recognized by Haeckel, and it was not really a problem of science: in discovering the 'fundamental law of the conservation of matter and force' science had done pretty much what it could do with respect to this problem – the rest that remained was metaphysics with which the scientist had no proper concern. Haeckel summarized his position as follows:

The number of world-riddles has been continually diminishing in the course of the nineteenth century through the aforesaid progress of a true knowledge of nature. Only one comprehensive riddle of the universe now remains – the problem of substance. ... [But now] we have the great, comprehensive 'law of substance', the fundamental law of the constancy of matter and force. The fact that substance is everywhere subject to eternal movement and transformation gives it the character also of the universal law of evolution. As this supreme law has been firmly established, and all others are subordinate to it, we arrive at a conviction of the universal unity of nature and the eternal validity of its laws. From the gloomy *problem* of substance we have evolved the clear *law* of substance.[15]

The basic structure of Haeckel's teaching is clear: science is rapidly nearing a state where all the big problems are being solved – specifically including the insolubilia of Du Bois-Reymond. (What remains unresolved is not so much a *scientific* as a *metaphysical* problem.)

The upshot of the controversy was well summarized by Karl Pearson:

We must here investigate a little more closely what the man of science means when he says, "*Here I am ignorant*". In the first place, he does not mean that the method of science is necessarily inapplicable, and accordingly that some other method is to be sought for. In the next place, if the ignorance really arises from the inadequacy of the scientific method, then we may be quite sure that no other method whatsoever will reach the truth. The ignorance of science means the enforced ignorance of mankind. I should be sorry myself to assert that there is any field of either mental or physical perceptions which science may not in the long course of centuries enlighten .... It is true that this view is not held by several leading scientists, both in this country and Germany. They are not content with saying, "We *are* ignorant", but they add, with regard to certain classes of facts, "Mankind must *always* be ignorant". Thus in England Professor Huxley has invented the term *Agnostic*, not so much for those who are ignorant as for those who limit the possibility of knowledge in certain fields. In Germany Professor E. Du Bois-Reymond has raised the cry, '*Ignorabimus*' ('We shall be ignorant'), and both his brother and he have undertaken the difficult task of demonstrating that with regard to certain problems human knowledge is impossible. (See especially Paul Du Bois-Reymond: *Über die Grundlagen der Erkenntnis in den exacten Wissenschaften*, Tübingen 1890.) We must, however, note that in these cases we are not concerned with the limitation of the scientist method, but with the denial of the possibility that any method whatever can lead to knowledge. Now I venture to think that there is great danger in this cry, "We *shall* be ignorant". To cry "We are ignorant" is safe and healthy, but the attempt to demonstrate an endless futurity of ignorance appears a modesty which approaches despair. Conscious of the past great achievements and the present restless activity of science, may we not do better to accept as our watchword that sentence of Galilei: "Who is willing to set limits to the human intellect?" – interpreting it by what evolution has taught us of the continual growth of man's intellectual powers.[16]

It is always risky to say *never*, and particularly so with respect to the prospects of knowledge.

AUTHOR INDEX

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